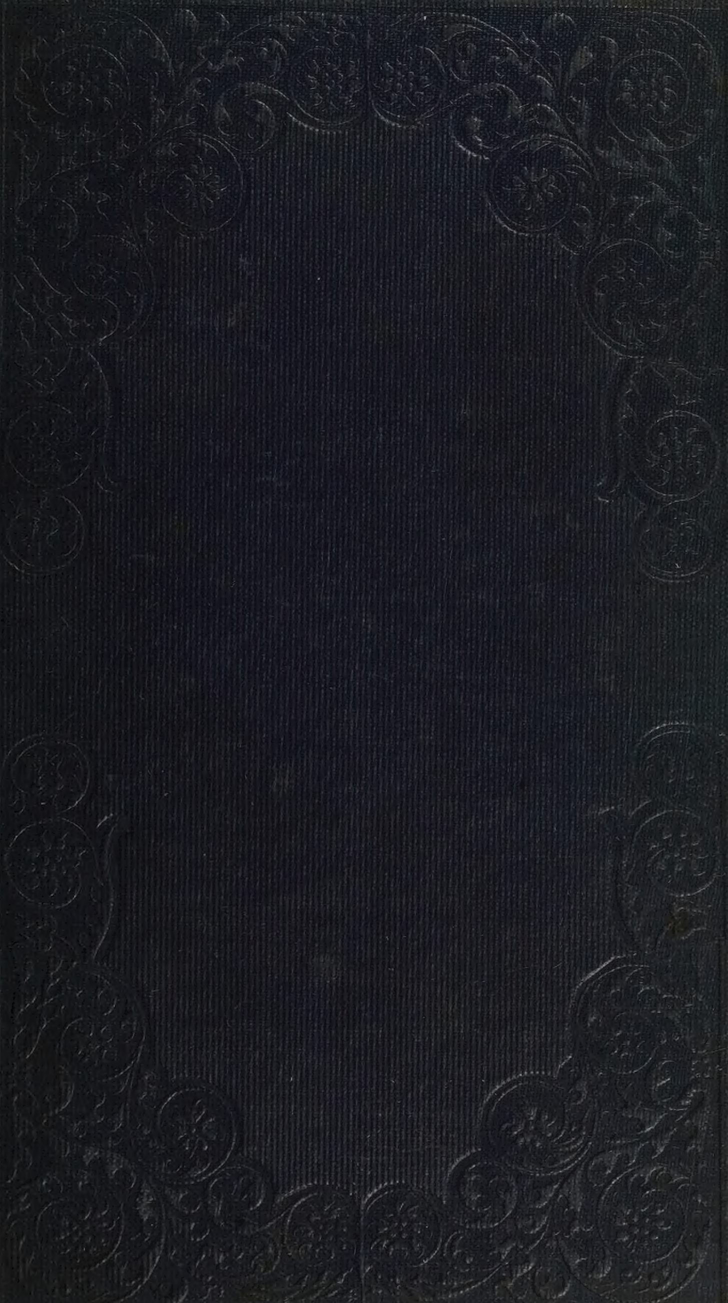

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BRAITHWAITE'S RETROSPECT.

VOL. LIV, JULY—DECEMBER, 1866.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN
AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D. LOND.

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CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER.—Dr. Broadbent looks upon cancer as primarily a local error of nutrition, in which not the blood but the part is at fault. It may be said to be constitutional, just as almost every disease is, in a sense, constitutional; but he does not believe in any condition to which the term "diathesis" is applicable, except such as is created by the existence of a tumour as a centre of dissemination. (p. 380.)

Destruction of Cancer by Injection of Acetic Acid.—Cancers can be treated by injection of acetic acid into their substance, either with the view of disintegration being followed by absorption without breach of surface, the tumour dwindling away gradually; or with a view of causing destruction of the growth with discharge of the debris. When the skin is involved, or when the tumour is of large size and rapid growth, absorption is not to be expected. When absorption is the end in view we must not inject too much acid, nor too strong, at once, and we must not repeat the injection for some time, for absorption is a slow process, often requiring months. When destruction is the object the injection should be most thorough, and the acid strong, so that the entire disease may be removed at once. When the skin is broken our best opportunity is gone, for the acid escapes and less effect is produced. (Dr. W. H. Broadbent, p. 377.)

Inject a dilute acetic acid into the centre of the tumour by means of a subcutaneous injection syringe. The needle should be introduced through the sound skin an inch or more from the part involved in the disease. About thirty minims of dilute acid (one part of acid to one and half or two of water) is the proper quantity for injection. Very little pain is produced. The whole tumour may be gradually destroyed by repetitions of this proceeding. When the larger portion of the tumour has been destroyed and a cavity produced, it may be stuffed with lint soaked in dilute acid until every vestige of cancerous matter has been destroyed. Dr. Broadbent considers large quantities of dilute acid preferable to stronger acid; and he would not, without great hesitation, attempt the destruction of any tumour which had not involved the skin. (Dr. W. H. Broadbent, p. 38.)

A case is related in which acetic acid was injected into a cancerous tumour of very large size, occupying the side of the face and neck. The patient was 73 years of age. Forty minims of acid, of the strength of one part in four, making 80 minims in all, were injected at two points. The operation gave no pain. More than one-half of the tumour was destroyed by this, and a large wound produced. The remains of the tumour only occupied the face, where numerous hard nodules could be felt. These nodules were treated separately by injections, and poultices containing acid were applied to the wound. At the period when the case was reported it could not be considered cured, but the wound was contracting and filling up with granulations. A mass was left over the maxillary and malar bones which it was difficult to treat by subcutaneous injections. (Dr. W. H. Broadbent, Dr. Randall, p. 228.)

Destruction of Cancer by Electricity.—By means of the chemical action of the continuous galvanic current, apart from the cauterising action of the same agent, a slow and gradual disintegration of living tissue may be brought about. This is applicable for the disintegration of cancerous tumours. The negative pole of the battery must be applied to the tumour and the positive at some distance from the diseased part. The parts acted upon form an eschar which slowly separates, without pain or reaction, leaving a soft and yielding cicatrix. (Dr. J. Althaus, p. 385.)

FEVER.—*Aconite as a Febrifuge.*—In cases in which there is increase in the heart's action, aconite may be employed with advantage as a febrifuge, instead of salines. It has in such cases the power of reducing the pulse and relieving the whole train of febrile symptoms. All cases characterised by nervous excitement and those in which there is palpitation are relieved by aconite. For an adult, two drops of the tincture (P. Lond.) every three or four hours is a proper dose. It may be mixed with powdered sugar and laid on the tongue. The sensation of "pins and needles" shows that the drug is acting on the system, and on the appearance of this symptom it should be discontinued or the dose diminished. (Dr. Prosser James, p. 288.)

Delirium in Fever, is it from head affection or from exhaustion?—When towards the termination of a case of fever the thermometer indicates a normal temperature, when perhaps the pulse is frequent, the patient constantly delirious, and the mouth black with sordes, it shows plainly that these symptoms are not the result of cerebral inflammation, but depend, in all probability, on exhaustion ; hence, nutrients and stimuli appear the rational curative agents, *versus* leeches, cupping, and other lowering remedies. (Dr. J. S. Warter, p. 3.)

GOUT.—*Hydrochloric Acid.*—A disordered digestion is the *primum mobile* of the whole train of morbid phenomena. This leads to imperfect performance of the function of respiration, whereby uric acid accumulates in the blood (it would otherwise be oxidized into urea, and excreted). Hydrochloric acid is a remedy of great value in some of these cases, it is the natural acid of the stomach, and when given along with food considerably increases the digestive power of the stomach. Alkalies certainly neutralize the uric acid already in the blood, but they do not go to the root of the matter. The hydrochloric acid is not put forward as a specific to be used under all circumstances and in every stage of the disease. (Dr. J. F. Duncan, p. 349.)

RHEUMATISM.—Dr. Bennett, of Edinburgh, has paid considerable attention to the question of the spontaneous cure of rheumatism, and he comes to the conclusion that it cures itself in about the same time, without any medicine, that it does when alkalies are given. If any shortening of the time is effected it is very inconsiderable. (Dr. J. Hughes Bennett, p. 36.)

Temperature in Acute Rheumatism.—In acute rheumatism we have a long continued slightly abnormal temperature, generally ranging between 99° and 102° Fah., and very rarely reaching 103° Fah. (Dr. T. A Compton, p. 7.)

RHEUMATIC FEVER.—*Permanganate of Potash.*—Permanganate of potash, which may be given in the form of Condy's fluid, is a remedy of considerable value in cases of rheumatic fever. It cleans the tongue, relieves pain, acts slightly on the bowels, and removes turbidity and fœtor from the urine when they have existed. The Condy's fluid should be diluted with seven parts of distilled water, and of this half-an-ounce given every second hour. (Dr. J. F. Duncan, p. 290.)

Quinine.—Give two grains of quinine combined with five grains of iodide of potassium four times a day. At the commencement of the case, let the patient have a vapour-bath of vinegar with subsequent cold douche. This is managed easily in bed, by wrapping up two almost red-hot bricks in coarse flannel soaked in vinegar, and placing one at either side of the patient on plates, but one near the shoulder and the other as low down as the knee. The cold douche is important in order to prevent the subsequent weakening sweats. It was pointed out by Dr. Heberden, more than a hundred years ago, that there is considerable resemblance between ague and rheumatism, in the rigors, hot and sweating stages, and in the tendency to periodicity in both diseases. (Dr. J. B. Nevins, p. 33.)

SCARLATINA AND DIPHTHERIA.—*Chlorate of Quina.*—From the powerful oxidising and general stimulating agency of chloric acid, and the influence of quina as a nervine tonic, Dr. Lyons has been led to the idea of combining these two remedial agents with the view of obtaining a febrifuge medicine of great potency. The results obtained from the use of this salt in doses of four grains every three hours in cases of scarlatina and diphtheria have so far been very satisfactory. It may be obtained from Messrs. Bewley and Hamilton, Dublin. (Dr. Lyons, p. 343.)

Scarlatina and Tonsillitis.—Diagnosis by Means of the Thermometer.—The diagnosis between tonsillitis and scarlet fever in their early stages is frequently very embarrassing, but we may derive much assistance by observing the temperature. In tonsillitis the temperature in the middle of the day, when at its maximum, which is generally attained about the fourth day, rarely exceeds 100°·5 Fah.; and this occurs with a pulse often below 100. Normal is reached about the sixth or seventh day. In scarlet fever, on the other hand, the maximum is arrived at about the third day, when the temperature is generally 104° Fah., whilst the pulse is considerably above 100. The normal is not usually attained until the tenth or twelfth day. (Dr. T. A. Compton, p. 7.)

TEMPERATURE IN HEALTH.—A temperature of 98°·4 Fah. is the point generally settled upon by the majority of authorities on the subject as the average in healthy adults. This is probably too high. The healthy range is somewhere between 95°·5 and 98°·5 Fah., the most common temperature met with being probably 97°·4 Fah., *i.e.*, one degree less than the temperature hitherto most generally received as the normal one. (Dr. T. A. Compton, p. 5.)

Effects of Alcohol on the Temperature of the Body.—Alcohol when taken by persons in health in ordinary quantities, causes a depression of the temperature of the body. The amount of depression is however slight and not sufficient to contraindicate its use. In poisonous doses the depression is more marked, amounting to about three degrees. From many observations made to ascertain the influence of alcohol on the temperature of febrile persons, it results that alcohol causes only a slight and temporary depression of their temperature, and consequently that it cannot bring the temperature of febrile patients to that of health. (Dr. Sydney Ringer and Dr. W. Rickards, p. 21.)

TYPHUS AND TYPHOID FEVERS.—*Thermometry in.*—The thermometer is, as its indications are becoming better understood, assuming an important place in our means of diagnosis and prognosis, both in fevers and most acute diseases. In both typhus and typhoid the temperature rises in a more or less regular manner to its maximum on the seventh or eighth day, when it gradually falls. In typhoid an abnormal course of temperature much more generally precedes a fatal termination than any unusually high range. The approach of any complication may frequently be foretold by a rise in the temperature. Fatal cases of typhoid have a temperature of about 104° Fah. as late as the sixteenth day, when, even in severe cases, the normal ought to be nearly reached. (Dr. T. A. Compton, p. 14.)

As a general rule the temperature of the body rises high in *typhoid* fever in the first week, while the pulse remains comparatively low, and this is not the case in typhus, or in many of those diseases for which this fever is apt to be mistaken in its early stage. In typhus fever the pulse and temperature rise together till the fever reaches its climax, when the pulse ranges generally from 110 to 144, and the temperature from 102° to 104°·5. There are, however, many exceptions to this rule; thus occasionally in febricula, acute rheumatism, tonsillitis, exhaustion from suckling, and phthisis pulmonalis, a temperature of 102° or 103° may occur with a low pulse-rate. Perhaps the cases of enteric (typhoid) fever in which the diagnostic use of the thermometer is most striking, are those in which malaise and perhaps some diarrhœa have existed for several days, and yet the patient has got about. Again, in many cases of supposed typhus, with history of exposure to infection, a rigor, a muddy skin, rapid pulse, and furred tongue, the fact of the temperature at mid-day being below 99°·5 or 100° is sufficient to enable us to assure the patient that he is really free from typhus. (Dr. J. S. Warter, p. 1.)

The most important rules that can be derived from the records of a single daily temperature are two; namely, that a sudden rise of temperature in either indicates some intercurrent disease, as congestion of the lungs, and that a sudden fall of the thermometer in typhoid fever indicates either diarrhœa or hemorrhage. (Dr. J. S. Warter, p. 3.)

In typhus the pulse and temperature rise more or less together until the seventh, eighth, or ninth day, when they are at their highest, then they begin to decline, and drop down steadily day by day in unison, both becoming normal about the fifteenth or sixteenth day. In typhoid fever, however, all is chaos; the pulse and temperature rise and fall irregularly, taking separate and distinct courses. Both, however, remain, on the whole, moderately high from about the sixth to the fifteenth day, then a fall takes place pretty rapidly, and between this and the twentieth day the normal mid-day temperature is again reached in most cases. (Dr. J. S. Warter, p. 3.)

AFFECTIONS OF THE NERVOUS SYSTEM.

DELIRIUM TREMENS.—*Capsicum.*—Let thirty grains of capsicum be made into a bolus and swallowed at one dose. It sometimes has an excellent effect, being of great and immediate efficacy. (Dr. Lyons, p. 82.)

EPILEPSY.—*Chloroform.*—Though theory at first contra-indicated the administration of anæsthetics, subsequent experience has fully established the fact, that, contrary to presupposed views, chloroform in full doses controls the epileptic fit, and while good breathing is kept up there is no cause of apnœa, or alarm from suffocation. (Dr. Kidd, p. 287.)

FACIAL PARALYSIS.—*Electricity.*—In employing electricity for the cure of Bell's paralysis of the portio dura, the induced current must be used and perseveringly employed by means of wet sponges, and pointed metallic conductors. (Prof. Aquila Smith, p. 306.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ANEURISM.—*Acupressure.*—Mr. Henry Lee proposes to employ acupressure for the treatment of popliteal and other aneurisms which are accessible to the needles. The popliteal artery may be compressed without much difficulty by this means. A successful case is related. Mr. Lawson Tate has treated aneurism in the way proposed by Mr. Lee for six or seven years. He dissects carefully down as for the application of ligature. If pressure is kept up too long suppuration may result. The essential condition for success is that the inner coat of the artery should *not* be lacerated, for if the two sides of the artery be kept together for a few hours they will unite. If the needle be passed through a vein embolism will probably result. (pp. 312, 315.)

HEART DISEASE.—*Epistaxis.*—Mitral narrowing has a greater tendency than any other valvular lesion to give rise to hæmoptysis. It may sometimes when taken along with other symptoms be an aid in diagnosis, especially as there is often wanting the murmur which is so audible and diagnostic in other forms of valvular disease. (Dr. Hyde Salter, p. 358.)

PULSE.—*Effects of Alcohol on.*—Alcohol increases the force of the pulse, but lessens its frequency. (Dr. S. Ringer, p. 22.)

PYÆMIA AND PHLEBITIS.—There is no necessary connection at all between pyæmia and phlebitis ; it is indeed very rare to see pyæmia following upon well-marked phlebitis during life, even when a vein and its branches are quite closed by coagulum or adherent fibrine. In the worst cases of pyæmia the veins are generally found perfectly healthy. In some cases of thrombosis a puriform fluid is found in the veins, this, however, is softened and disintegrated fibrine, and is, although to the eye almost indistinguishable from pus, really quite distinct from it, and possibly a *post-mortem* effect in some cases. (Mr. W. S. Savory, p. 256.)

THROMBOSIS AND PHLEBITIS.—Phlebitis is an inflammation which really affects the walls, and not the contents of a vessel. In the larger vessels the different layers of the walls become inflamed and thickened, but all the while the channel may remain perfectly unaltered. No experiments have succeeded in producing a deposit in the cavity of the vessel when all blood is excluded from its interior. On the other hand, in most cases of thrombosis there is no mark of inflammation in the coats of the vein, and in all the deposit on its walls can be readily peeled off, which it could not be were it an exudation from the walls of the vessel. Thrombosis, then, may occur without phlebitis, and phlebitis without thrombosis ; but thrombosis without phlebitis is of more frequent occurrence than phlebitis without thrombosis, for phlebitis is a frequent cause of thrombosis. Thrombosis sometimes occurs without any explainable cause, the vein being perfectly healthy, and no other possible cause assignable. A thrombus is distinct from an embolus : the former is a clot formed in a vein ; the latter is a piece of detached fibrine from a clot formed in a vein, or from a deposit on the valves of the heart, carried into the circulation and blocking up some artery into which it is carried. Virchow's theory of pyæmia (probably erroneous) is, that embolism always precedes pyæmia, and that the numerous secondary abscesses are formed by small granules of fibrine blocking up distant capillaries and there softening and forming abscess. (Mr. W. S. Savory, p. 262.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

Asthma.—There is a great difference, generally, between the causes of the disease and that of the paroxysm. The disease itself is generally excited first of all by bronchitis, and subsequent attacks of bronchitis may produce fresh paroxysms of the asthma. The excitants of the paroxysm may be divided into direct excitants, as things inhaled, an inflammatory condition of the bronchial mucous membrane, the presence of a blood—such as *sanguis cibi*—the blood after a meal, which though not itself necessarily morbid, yet in the peculiar pulmonary system of the asthmatic, constitutes a source of irritation. The other division of excitants includes those which are applied to the periphera of the nervous system, as an indigestible meal, a loaded rectum ; or such as affect the brain itself—either physical or psychical. The presence of animals, such as cats, dogs, horses, &c., has, by the effluvium from their bodies, sometimes the effect of inducing an attack of asthma in those subject to the disease. This peculiarity may be quite unsuspected by the patient. An instance is given in which the proprietor of an equestrian establishment was constantly subject to attacks of the disease until he retired from the business, when he became free from them, but if he goes back to his old haunts among the horses his old troubles recommence. Another gentleman was unable to sleep in any house in which was a dog.. (Dr. Hyde Salter, p. 103.)

Catching Cold.—Cold does not so much arise from exposure to cold as from going from a moist cold atmosphere into a hot dry one. In coming home after being out in cold inclement weather, it is well for a person liable to catarrh to loiter a little in the hall before going into a heated room. (Dr. T. Inman, p. 33.)

Diphtheria.—When the exudation is fairly manifested, the most efficient local application is strong hydrochloric acid diluted either with honey or with water, in equal bulk. This may require repeating once or twice again, at an interval of twelve or twenty-four hours. If much fœtor exists, a tolerably strong solution of chloride of zinc may be applied to the throat, and a solution of permanganate of potash employed to cleanse the mouth from stringy saliva and offensive discharges. The use of nitrate of silver as a caustic is unsatisfactory. The class of internal remedies from which most good may be expected is preparations of iron, given from the very commencement of the disease. The ordinary tincture of the sesquichloride is for several reasons the most suitable preparation. Stimulants should be used early and without hesitation; as, in cases of fever, the first-noted feebleness of the heart-beat, or softness and rapidity of the pulse, furnish full reasons for their employment. (Dr. W. Newman, p. 109).

HEMOPTYSIS.—*Insufflation of Dry Sulphate of Iron.*—In a remarkable case of hemoptysis related, the repeated insufflation of small quantities of persulphate of iron, very dry, and reduced to an impalpable powder, completely arrested the hemorrhage. Lead, opium, and the usual remedies had failed to give relief. (Dr. W. A. Wetherby, p. 171.)

HOOPING-COUGH.—In some aggravated cases of hooping-cough the inhalation of chloroform proves very beneficial. (Dr. C. Kidd, p. 286.)

PHTHISIS.—*Hygienic Treatment.*—Many medical men instead of regarding the progressive deposit of tubercle in the lung, with its preceding or accompanying bronchial and laryngeal symptoms, as mere local manifestations of a general diathesis, exaggerate their inflammatory nature, and do not apply to their patients the ordinary rules of hygiene. They dare not give wine and plenty of animal food; they dare not give fresh cool air day and night; and they dare not keep the skin cool and clean by cold or tepid sponging. All consumptive patients, whatever their condition, if they have the strength, should use a sponge-bath at a temperature of from 64° to 68° daily. The greatest possible benefit will be derived from this, the pulse will be lowered, and a feeling of indescribable comfort experienced. Dr. Bennet states that he himself, when his pulse was 100 and skin hot and feverish, used to sponge in summer in the open air on the banks of a Scotch loch, the waters of which were at 60°, and derived therefrom the greatest benefit. (Dr. J. Henry Bennet, p. 94.)

Phthisis and Diseases of Debility.—Raw Meat and Brandy.—The author has been treating consumptive diseases in France by raw meat and brandy, and, he states, with wonderful success. It has now been tried in 2,000 cases, and in nearly all successfully. The patients soon begin to increase in weight. He also recommends the adoption of his treatment for the following maladies: advanced anæmia, the last stages of ague, typhus and typhoid, leucocythæmia, albuminuria, and diabetes, and also in cases where there has been great loss of blood or seminal fluid. (M. Fuster, p. 99.)

TRACHEOTOMY.—*A New Instrument for.*—Mr. Worthington, late house-surgeon to the Middlesex Hospital, has invented a deeply-grooved steel director, which is closely fitted to the back of the scalpel used in opening the trachea. It is left in the trachea when the scalpel is withdrawn, and serves to facilitate what often proves one of the most difficult steps in tracheotomy—the introduction of the canula into the trachea. (p. 363.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Anal Fistula.—Pass, by means of a probe, a piece of stout thread or ligature through the fistula into the bowel, and bring the end out at the anus. Tie the two ends together loosely, and after a time tighten as necessary. The thread will ulcerate its way out, the parts in the rear healing as the thread advances. Sea-water injections used at the same time are of great use. (Dr. Andrew Paul, p. 185.)

Cholera. — *Camphor.* — Alcohol when 60° above proof will dissolve its own weight of camphor. This solution has been used by Dr. Rubini, of Naples, in 592 cases without a single death. The method of cure is as follows: When a man is seized with cholera, he should at once lie down, be well wrapped up in blankets, and take every five minutes four drops of the saturated tincture of camphor. In very severe cases the dose ought to be increased to from five to twenty drops every five minutes. In the case of a man of advanced age, accustomed to take wine and spirits, where the drug given in drops has no effect, give a small coffee-spoonful every five minutes, and in a very short time the coveted reaction will occur. Ordinarily, in two, three, or four hours abundant perspiration will come out, and then cure will follow. The "preventive method" is this, let those who are in good health, while living in accordance with their usual habits, take every day five drops of the saturated spirits of camphor upon a small lump of sugar (water must never be used as a medium, or the camphor will become solid, and its curative properties cease), and repeat the dose three or four times a-day. Spices, aromatic herbs, coffee, tea, and spirituous liquors should be avoided. (Dr. Rubini, Mr. F. Smith, p. 399.)

Fifteen cases of cholera are reported at the Whitechapel Workhouse Infirmary which were treated with camphor dissolved in an equal weight of chloric æther. Fifteen drops of the solution were given on sugar in the first instance, and ten drops every quarter of an hour afterwards, according to the urgency of the symptoms. In about half the cases the extremities became warmer and lost their lividity, bile reappeared in the evacuations, and reaction took place. (Mr. Widdas, Mr. Richardson, p. 398.)

Nitrous Acid.—The diarrhœa premonitory of cholera may be speedily arrested by the administration of nitrous acid, and this suppression is never followed (in uncomplicated cases) by the very unmanageable consecutive fever which usually occurs when it has been effected by opium alone, or combined with the usual astringents. Moreover, in cases of collapse, when vomiting and purgation are at an end, it will cause reaction when no other remedy—not even castor-oil—will avail to do so. The best formula is a drachm of nitrous acid in eight ounces of water, of which a table-spoonful may be taken in a wine-glassful of water after every liquid stool. If there is also vomiting, it should be combined with gallic acid. (Dr. G. E. Nicholas, p. 396.)

Feeding by the Veins.—Dr. Richardson, of London, has invented a most beautiful and delicate instrument by means of which patients may be fed by the veins with a solution of digested albumen. The use of this instrument in cholera is likely to be very great. The albumen is digested by exposure for a very long time to a low degree of heat—not sufficient to produce coagulation. (Dr. B. W. Richardson, p. 307.)

GALL-STONES.—*Passage of.*—The inhalation of a few drops of chloroform may be of the greatest service in cases of obstruction of the ductus communis by a gall-stone, relaxing the muscular fibres of the duct, and allowing of the passage of the stone. Purgatives and opium would probably make such cases worse. (Dr. C. Kidd, p. 286.)

HEMORRHOIDS.—*Arsenic.*—It is now upwards of thirty years since Sir Charles Locock pointed out the value of arsenic in *atonic menorrhagia*, and Dr. Handfield Jones explains the results by the influence arsenic exerts in producing contraction of the blood-vessels. It is singular that arsenic exerts a wonderful power in cases of hemorrhoids with or from atony of the system. Doubtless it acts upon the enlarged hemorrhoidal vessels, although its application in the treatment of such diseased states is entirely novel. (Dr. T. Parvin, p. 179.)

HERNIA.—The old mode of treating hernia is now fortunately given up, and the safe and successful operation without opening the sac is more promptly resorted to, and with a considerable reduction in the mortality per cent., which was often as high as two deaths in three. Purgatives were administered by mouth and rectum, the lowering effects of bleeding, tartar emetic, and tobacco were, one or other, or all, judged necessary, and prolonged attempts at reduction by taxis were repeated from time to time. The great mortality was due to the combined influence of delay, of depression resulting from medicines, and of actual injury done to the intestine by the taxis. (Dr. M. H. Collis, p. 175.)

The Taxis.—It must be borne in mind in employing the taxis in cases of hernia that the object is not to return the bowel, but to empty it, to unload it, if possible, first of its gaseous contents, and then of its feculent. (Dr. M. H. Collis, p. 176.)

Femoral Hernia.—Gay's operation for the relief of strangulated femoral hernia is beautifully simple, and as safe as it is simple. Make an incision parallel to Poupart's ligament and just above the neck of the sac, towards which the finger of the operator must be directed. The finger-nail must be used to pick up on the side of the sac every constricting fibre which intervenes between the surface and Gimbernat's ligament. In recent hernia there will generally be found one tolerably well-marked superficial constriction, firm enough to be taken for Gimbernat's ligament. This must be carefully divided with the hernia knife. When all fibres and bands are divided or ruptured, the true stricture is reached at Gimbernat's ligament; it lies wonderfully deep down, and it is to be remembered that the finger must be pushed on at right angles to the place of the external wound, not being suffered to pass in any degree upwards, else the stricture will be missed and the finger be lost in a region above and superficial to it. Once felt it cannot be mistaken; a touch of the knife divides it enough, a slight nick will do. Gentle compression with one finger in the wound and the others outside upon the sac, will empty the latter, and complete the reduction. (Dr. M. H. Collis, p. 177.)

Inguinal Hernia.—A case is related in which the author succeeded in curing a case of reducible inguinal hernia by a new method of operation. The internal spermatic fascia or fascia propriâ of the hernia having been exposed by incisions at right angles to Poupart's ligament, an aneurism needle was carefully insinuated through the areolar tissue of the lower and deeper part of this fascia, and by its means a ligature was placed between the sac of the hernia and the important structures of the spermatic cord, and carried through the upper and deeper part of the fascia. The fascia and sac were then drawn gently downwards and towards the median line of the body, and the ligature was tied tightly as high up and as far outwards as possible; in fact, as nearly as could be judged, exactly at the internal or deep ring. The ligature thus included the greater part of the circumference of the fascia propria or internal spermatic fascia just where it becomes continuous with the fascia transversalis, the whole circumference of the sac just at its junction with the general peritoneal lining of the abdominal parietes, and within the sac a small plug-like portion of omentum. (Mr. A. E. Durham, p. 174.)

A simple method of curing reducible inguinal hernia consists in sewing the columns of the inguinal ring together, subcutaneously, by silver wire, and leaving the wire permanently in the tissues, so as to act the part of a permanent internal clamp. The only instrument needed is a stiff needle five inches in length, very slightly curved towards its point, near which is placed the eye. The other extremity of the needle is secured in a firm handle, which enables the surgeon to control its movements. (Dr. J. J. Chisolm, p. 172.)

Hydatids of the Liver.—*Injection of Extract of Male-Fern.* A case is recorded at Guy's Hospital in which a large fluctuating deep-seated tumour existed on the right side, which was diagnosed as hydatid tumour of the liver. About four ounces of a limpid, colourless fluid were allowed to escape through a fine canula, which, with a trochar, was introduced into the swelling. A liquid, consisting of half a drachm (by measure) of the purified semi-fluid extract of male-fern, half a drachm of liquor potassæ, and six drachms of water was then injected into the sac, care being taken throughout to prevent the admission of air. The affection was permanently cured by this treatment. (Dr. F. W. Pavy, p. 112.)

AFFECTIONS OF THE URINARY ORGANS.

Diabetes.—Diabetics do better on a natural than upon a restricted diet, saccharine and amylaceous food being as necessary to their comfort and well-being as to that of persons in health. The circulation of sugar in the blood is not productive of bad symptoms either immediately or remotely, and the tendency to it is not decreased by "diabetic diet" however long persevered in, for the ingestion of amylaceous matter is always followed by a discharge of sugar from the system. (Dr. G. Owen Rees, p. 117.)

The system of dieting in cases of diabetes, especially in patients advanced in life, is not always required to be permanently persisted in. It often, and indeed in old people generally, happens that the healthy state becomes thereby so restored, that an ordinary diet can be taken without any appearance of sugar, or only a little, in the urine. This, however, only happens after perseverance for a few months or a year. Dr. Pavy, whose observations the above are, has lately seen a case in a lady sixty-five years of age, where the sugar disappeared from the urine in a week, whereas previously to following the restricted diet prescribed for her, she had been passing forty-eight grains of sugar to the ounce. (Dr. F. W. Pavy, p. 121.)

Bran Biscuits in Diabetes.—The bran biscuits so much used in diabetes, contain 28 per cent. of starch—this renders their employment quite nugatory. The bran should be prepared by steeping in water, to which a little sulphuric acid is added, which will convert all the starch into glucose or grape sugar; this may be washed away and the bran thus rendered quite free from starch. (Dr. A. Hill Hassall, p. 123.)

DISTENDED BLADDER.—*To feel per Rectum.*—In cases of great distention of the bladder you cannot always feel it above the prostate per rectum. It is a singular fact that the fuller the bladder the less you can generally feel it by the rectum. You can, however, always succeed by pressing the bladder down from the front of the abdomen into the pelvis with one hand, the finger of the other being placed at the same time within the rectum. (Mr. J. Hilton, p. 297.)

LITHOTRITY.—*Instrument for the Removal of fragments of Calculus after.*—This instrument consists of a large silver catheter having an oval opening on the convex surface. Within the straight part of the tube works a piston, by the partial withdrawal of which the sedimentary matter is sucked into the oval opening. Attached to the extremity of the piston-rod is a piece of watch-spring which ends in a small scoop, and three other small scoops or catches are situated on the spring nearer the piston. As the piston-rod is further withdrawn these catches remove from the interior of the tube the particles of stone sucked into the opening by the piston. (Mr. J. Wood, p. 218.)

PAROXYSMAL HÆMATURIA.—Cases occasionally occur in which the urine usually is quite free from albumen, and natural in every way, but at certain irregular intervals, and always following exposure to cold, it contains a large amount of albumen, and sometimes blood globules. When the hands and feet get chilled, the patient is seized with shivering, and begins immediately to pass urine more or less dark coloured from the presence of blood. It may be assumed that in these cases there is no organic disease of the kidneys, as the urine between the attacks is quite healthy. The explanation must be, that in these cases the circulation in the kidneys is affected by the exposure to cold, just as under similar circumstances one person would have coryza, another bronchitis, another muco-enteritis. There are varying degrees of susceptibility to cold appertaining to different parts of different individuals. The principle to be acted on in the treatment is first of all, by suitable coverings or clothing, to diminish the risk of the patient getting chilled, and next, to fortify the system by tonics and render it less impressionable by a narcotic. (Dr. F. W. Pavy, p. 114.)

Retention of Urine from Drinking.—In a case of retention of urine from stricture spasmodically contracted from drinking, seat the patient over a shower of hot poppy-head decoction, which may be forced in spray by a suitable douche. In a variable time (twenty minutes in a case related) the bladder will be emptied almost involuntarily, and the use of the catheter rendered unnecessary. (Dr. Andrew Paul, p. 181.)

Stricture of the Urethra.—In the ordinary plans of treating stricture of the urethra we distend the narrowed part to the same size as the external meatus, whereas that portion of the urethra which is most frequently affected by stricture possesses in its natural condition a calibre at least double that of the external meatus. We ought to distend the strictured portion to at least twice the size of the meatus, and to apply the distending force to the strictured portion alone. This can be accomplished with ease by means of Mr. Thompson's instrument. This instrument is composed of two parallel rods, which can be slowly separated by means of a screw in the handle, through the action of two levers which exist between them. When opened the two blades produce the figure of a very elongated spindle, the centre of which corresponds with the site of the stricture. The instrument having been inserted, from seven to ten minutes must be occupied in slowly distending the part, the principle being to overdistend as much as possible and to rupture as little as possible. For an old and non-dilatable stricture within two or three inches of the meatus, internal urethrotomy should be preferred. If the urethra is narrowed at several points, Mr. Holt's plan is to be preferred, as more certain to hit all the points of narrowing. Mr. Thompson considers his own plan only suitable for those strictures, forming a large majority, which exist at from four to six inches from the meatus, and employs it only in the most intractable cases, preferring always to employ ordinary dilatation (usually by tying in the gum catheter) when it is possible to accomplish his object by this simple process. (Mr. Henry Thompson, p. 196.)

The ordinary plan of operating in cases of impermeable stricture, viz., by effecting a union between the two permeable portions of the urethra, by cutting down upon and dividing the intermediate strictured or obstructed portion—is most unsatisfactory, being frequently not only unsuccessful, but dangerous. Mr. Cock, of Guy's Hospital, says, "I have frequently seen this operation performed, or rather attempted; and I doubt whether in any one instance the original intentions of the surgeon were carried out." When a passage has really been established by the wound healing over the catheter, the canal which is formed is only a factitious one and begins to contract as soon as the use of the instrument is abandoned. Mr. Cock considers a preferable operation to be that of tapping the urethra at the apex of the prostate. The only instruments required are a *broad* double-edged knife, with a very sharp point; a large silver probe-pointed director, with a handle; and a canula, or a female catheter, modified, so that it can be retained in the bladder. The patient being placed in the lithotomy position, and the left forefinger of the operator being lodged, through the rectum, on the apex of the gland, let the knife be plunged steadily, but boldly, into the median line of the perineum, half an inch in front of the anus, and carried on to the apex of the prostate. It is of the utmost importance that during the operation the knife be not removed from the wound, and that no deviation be made from its original direction until the urethra is opened. The external wound should be prolonged a little upwards by a vertical motion of the knife, so as to be wider than the internal apex of the wound. A canula must be left in the wound and secured there by four pieces of tape. By this plan immediate relief is obtained, and the sinusees in the perineum are allowed to heal. The surgeon will, after a time, often be agreeably surprised to find that the sound passes readily through the old stricture, and that by proper treatment its calibre may be restored. (Mr. E. Cock, p. 210.)

New Dilator.—Dr. Aspray, of London, describes his new instrument for the cure of stricture of the urethra as combining the safety of the gradual method with the quickness of action and speedy cure of the rapid dilatation principle. (It is figured at page 194). A No. 1 catheter is first passed into bladder, and serves as a guide to a dilator which is passed over it to the stricture. The dilator terminates in a conical screw which is screwed through the narrowed portion of the urethra. The dilator is then withdrawn, and a bougie a little smaller than the dilator passed in its stead over the guide and allowed to remain. In many cases, however, the bougie is not required. (Dr. C. O. Aspray, p. 192.)

AMPUTATIONS, FRACTURES, DISLOCATIONS, &c.

Amputation about the Foot and Ankle.—In operations of the foot for removal of diseased bone, it is the usual plan to remove bones at their articulations instead of simply removing the diseased portions ; for instance, if the anterior surface of the scaphoid is diseased, the whole bone would be removed, whereas the removal of a slice off the front would suffice. Where practicable, we should cut through the tarsal bones with a saw in preference to disarticulating them, and a rule should be followed of avoiding the destruction of joints whenever we can do so. (Mr. H. Hancock, p. 130.)

When it is necessary to remove any of the anterior portion of the foot more forward than Chopart's operation, it is well to ignore the joints, and to saw through the bones at the point of election. The foot is an aggregation of several parts, each perfect in itself and of too great value to be destroyed without a cause. (Mr. H. Hancock, p. 129.)

Subastragaloid Amputation.—Malgaigne's method of operating (or more correctly M. Lignerolles') in cases where it is necessary to remove the foot, possesses advantages which cannot be claimed either for amputation at the ankle-joint, for Syme's, or for Pirogoff's operation. The plan is to remove the os calcis with the foot, and to leave the astragalus in situ, whereby a broad, firm, flat surface is obtained at the end of the stump, covered with natural heel structure. The removal of the projecting malleoli as in amputation at the ankle-joint, is avoided. The flaps must be made from the sides of the foot, and be reflected towards the malleoli before the foot is disarticulated. The dorsal incision must be made over the medio-tarsal joint, instead of being carried in front of the ankle-joint. By this the sheaths of the extensor tendons are left undisturbed, and the risk of suppuration in the course of the tendons is materially diminished. The length of limb is equal to that given by Pirogoff's operation, but whereas in the latter this increased length of limb is obtained by a piece of bone transplanted, so to speak, to new ground, to an abnormal situation, the former supplies it by a non-disturbance of parts, by the preservation in fact, of the astragalus in its natural position. (Mr. H. Hancock, p. 143.)

There is a modification of subastragaloid amputation by which in some cases the length of the limb may be still further increased. It is a modification of Pirogoff's. Leave the ankle-joint intact, and remove the head and under surface of the astragalus, and the whole of the os calcis with the exception of the posterior third. This posterior portion should be adapted by its cut surface to the under surface of the astragalus. (Mr. H. Hancock, p. 151.)

CARIES OF THE VERTEBRÆ.—In caries of the dorsal vertebræ, allow the patient to assume that position which is most comfortable, namely, the semi-flexed position ; a water bed will be required. This position favours that coalition of the bones whereby alone a cure can result. (Mr. J. Adams, p. 187.)

CLUB-FOOT CURED WITHOUT TENOTOMY.—The ordinary cause of deformity of the foot is not contraction of certain tendons, but paralysis of the muscles connected with the uncontracted ones. Hence division of tendons which retain their normal power is not the way to cure the deformity. The author's mode of treatment is to supply the place of the paralysed muscles by means of elastic cords. By a light and simple contrivance these cords are fastened to points as near the origin and insertion of the paralysed muscles as possible, and the patient is encouraged to use his limb, while at the same time constitutional remedies are prescribed, and local means, such as friction, galvanism, &c. are employed. This treatment proceeds on the principle that our aim ought to be to strengthen weak parts, and not to weaken strong ones. The author has adapted his method to the treatment of knock-knees and crooked shins; and he has devised means by which spring force may be brought to bear upon these deformities in such a way as to draw the bones into a more natural position. (Mr. R. Barwell, p. 166.)

DISLOCATION AT THE SHOULDER-JOINT.—Dislocation at the shoulder may be very conveniently reduced by causing the patient to mount to a convenient height on some "steps," placing his arms across the top step, upon which is placed a pillow. The top step acts as fulcrum, and the weight of the patient's body answers to a great extent as counter-extension. (Dr. G. Hamilton, p. 163.)

Dislocation of the Hip and Shoulder.—Reduction by Manipulation. — The most important condition to be ensured for reducing dislocation of the hip is a relaxed, but not perfectly helpless, flaccid, uncontractile condition of the muscles. At the first commencement of manipulation, particularly in cases of recent date, the movements made with the limb should be very gentle, not rough, nor abrupt. After flexing the limb well, it should be suddenly and rapidly, but not violently, rotated on its axis, and completely abducted, or adducted, as the case requires, and at the same time depressed or raised, when the adjuvant muscles will be perceived to rapidly contract. A snap is commonly but not invariably heard, and the reduction is accomplished. Of anæsthetics, the Dutch liquid (chloride of olifiant gas) or the bromide of ethyl are preferable to chloroform, as they have less tendency to induce muscular contraction and that violent struggling which chloroform so commonly induces before the system is brought fully under its influence. (Mr. T. Nunneley, p. 161.)

ENLARGED BURSA OF THE PATELLA.—Puncture the bursa with a lancet, and after carefully expressing all the contents, apply firm pressure by a pad of lint, strapping, and bandage, so that its walls may be everywhere kept in contact, and its cavity obliterated. The case is generally cured at once and permanently, but the bursa may refill. In this event the same process must be repeated, and it is sure to succeed in the end. Even if repetition is necessary, a cure will be procured more easily, surely, and quickly than by any of the ordinary plans of treatment. (Mr. W. S. Savory, p. 169.)

FRACTURES.—*Liquid Glass Bandages.*—Liquid glass is silicate of potash, which is soluble in hot water, but insoluble in cold. The solution, applied by means of a brush over the ordinary bandages, rapidly hardens, and gives all the advantage of plaster of Paris or starch, with the advantage of being lighter and more cleanly. It is readily removable by means of hot water. (Prof. Schuh, p. 287.)

Fractures of the Spine.—Trephining.—The operation of trephining in cases of fracture of the spine, is perfectly practicable, and may in some cases be of use. An incision from four to five or six inches long must be made through the integuments over the spine, with a strong scalpel, and by means of a strong curved bistoury the muscles must be divided on each side of the spinous processes, and a retractor with a broad flange inserted at each side. The operator should next feel in the sulcus, on each side of the spinous process, for the inequality caused by the injury, and by grasping successively each spinous process, in a pair of necrosis forceps, ascertain whether the posterior portions of any vertebra are broken. In the lumbar and lower dorsal region the division of any laminæ is extremely difficult—it is accomplished by strong forceps bent at an angle, something like the forceps known as "Fergusson's side cutting forceps." Experience shows that the laminæ may be cut through without fear of injuring the cord or its membranes. In the cervical region the removal of the bone can be effected with great facility by the gouge-forceps alone, the spinous process being first removed by the same instrument. (Dr. R. M'Donnell, p. 157.)

Knee-Joint Disease in the Upper Classes.—Three-fourths of all knee cases in the upper classes of society are not cases of inflammation, though they appear so. There is no organic disease whatever in the joint. They are cases of local pain, originating in impaired health. The patient walks lame, the joint is stiff, not that it will not bend, but the movement is painful. There may be some increased heat in the joint when compared with that of the opposite limb, but not much in degree. The knee is slightly swollen. After the application of blisters, leeches, iodine, and other similar things useful in inflammatory affections, but injurious in these cases, the swelling of the joint will be palpable; the outline will have undergone a change. Months will pass in this way, the joint being neither stiffer, hotter, nor more swollen than it was in the earlier part of the case. Thoroughly nutritious diet, wine, bark, iron, sea-air, and change of scene, are the real remedies in such a case. (Sir B. Brodie, Mr. F. C. Skey, p. 65.)

AFFECTIONS OF THE SKIN, ETC.

Eczema of the Scalp.—Paint the scalp occasionally with strong tincture of iodine made with methylated spirit, at the same time keeping the hair cut short by repeated use of the scissors. The general health must not be neglected, cod-liver oil being given if possible. (Dr. F. W. Belcher, p. 218.)

Inflammation and Abscess.—It is a prevailing error to refer all examples of redness of vessels to inflammation. No term can be more inappropriate. The ancient definition of inflammation is founded on truth, but in the too frequent employment of the name we forget the conditions essential to it. Inflammation and abscess often result, as is so frequently seen in abscess in the breast after labour, from failure of nervous power and consequent unhealthy tone of the capillary system. There are two agents in addition to good nourishment especially applicable to the treatment of cases of this nature, bark and wine. In all cases of abscess bark is invaluable. It promotes suppuration when already inevitable, and it checks suppuration in chronic abscess. It promotes appetite, and gives vigour to the system beyond any known tonic. (Mr. F. C. Skey, p. 221.)

Measles.—*Thermometry in.*—In measles the range of temperature is a short and rapid one, thus contrasting well with typhus fever, for which this disease is apt to be mistaken. It rises to its highest point about the fourth, fifth, or sixth day, and then sinking rapidly it falls to the temperature of health about the seventh or eighth. Thus after the sixth day at any rate its course is diametrically opposite to that of typhus. (Dr. J. S. Warter, p. 3.)

SCABIES.—*Paraffine.*—Wash the parts well with a strong warm solution of common soda, and after drying apply paraffine very freely. A few repetitions will generally cure the case. (p. 221.)

VACCINE LYMPH.—*Dilution with Glycerine.*—Vacine lymph may be diluted with glycerine to a considerable extent without impairing its utility. While augmenting the vaccinating material ten or twenty-fold, the pustules which result differ in nothing from those produced by pure lymph. (Dr. Müller, p. 226.)

VENEREAL AFFECTIONS.

COPAIBA.—*To Deprive of its Disagreeable Smell.*—Copaiba and pitch of each one ounce; magnesia, a sufficient quantity to make a mass. According to the amount of magnesia, the mass will be more or less consistent. If the latter, a teaspoonful may be given two or three times a-day; if the former, pills may be made. M. Beyran, of Paris, has found this effectual to prevent the usual very offensive odour of the copaiba. (p. 240.)

GLEET.—*The Endoscope.*—The endoscope continues to be used in cases of urethral disease. It is especially useful in cases of gleet. The part of the mucous membrane affected may be seen distinctly, and a solution of nitrate of silver applied with very beneficial results. (Mr. C. Heath, p. 190.)

GONORRHŒA.—Never allow the patient to put a piece of lint or cotton-wool upon the mouth of the urethra to prevent the escape of the discharge, by which plan no doubt the shirt is kept clean, but the retention of the gonorrhœal fluid within the urethra ensures not only the intensification but the extension backwards of the inflammatory condition. (Mr. J. Hilton, p. 203.)

Oil of Erigeron.—The oil of erigeron is a volatile oil obtained from the E. Canadense, or Canadian fleabane, a plant officinal in the U.S. Pharmacopœia. It is a remedy of great value in gonorrhœa. It arrests the discharge in about seventy-two hours, and effects a cure in from six to eight days. If the urethral inflammation is severe, it is well to precede the remedy with a full dose of some active hydragogue, as jalap and senna. When this has operated, give ten drops of the oil on sugar, and three hours later, a full dose of spts. æther. nit. in infus. althea, and so on every three hours alternately, until the urethral irritation is allayed. Then leave off the latter, and continue the oil until the cure is completed. (Dr. J. S. Prettyman, p. 237.)

Oil of Yellow Sandal Wood.—Oil of yellow sandal wood may be considered almost a specific for gonorrhœa and gleet, it may be used in any stage of gonorrhœa. It is unfortunately very expensive. The following are the best formulæ:— ℞. Olei santal. flav., ʒiij.; ess. cinnamomi ʒj.; spt. rect. ad ℥ij. Misce. Dose, a tea-spoonful three times a day, in a wine-glassful of water, after food. ℞. Olei santal. flav., ℥i.; olei sabinæ, ʒj.; ol. cubebæ, ℥ss.; ol. copaibæ, ℥j.; ess. cinnamomi ʒj.; spt. rectif. ad ʒvi. Misce. Dose, same as preceding. (Dr. H. S. Purdon, p. 237.)

AFFECTIONS OF THE EYE.

Lacrymal Disease.—*Removal of the Lacrymal Gland.*—It is frequently impossible to cure cases of lacrymal disease, even by Bowman's plan of dilatation. A much more satisfactory mode of treatment in some cases (not all) is the removal of the whole or a portion of the lacrymal gland. This can be readily accomplished from the palpebral surface of the lid. The incision must be made close below the eyebrow, at the upper and outer part of the orbit. It is a singular fact that the lacrymation is cured whether the gland is completely or only partially removed. The eye continues moist after the operation, quite as moist in fact, as a healthy eye. (Mr. J. Z. Laurence, p. 230.)

Rheumatic Iritis.—In this, as in other forms of iritis, our chief aim should be to arrest inflammation, and so prevent effusion of lymph. Nothing answers so well as mercury, which is best administered in the form of calomel, combined with a quarter to half a grain of opium to each dose. Even when lymph has been effused and is becoming slowly consolidated, the administration of mercury will cause its absorption, provided it is given for a sufficient length of time. In persons however of broken-down health, or phthisical tendencies, a combination of two grains of quinine, three of blue-pill, and three of extract of hyoscyamus is more beneficial. Atropine is a useful auxiliary, but cannot supplant the use of other means of treatment. (Mr. J. Rouse, p. 233.)

MIDWIFERY, ETC.

Abortions.—"*Retained Placenta*" *in.*—It is quite unnecessary to meddle with the placenta after the fœtus has come away prematurely. It is often retained for many hours, and if the patient is plugged, she may safely be left to nature. On removing the plug in ten or twelve hours, the placenta will come away with it. (Dr. C. B. Suckling, p. 249.)

Cancer of Uterus and other Parts.—*Application of a Spirituous Solution of Bromine.*—Dr. Wynn Williams, of the Samaritan Hospital, London, for some time has been using a spirituous solution of bromine as an application to any cancerous growth where there is a breach of surface. The bromine and spirit must be mixed very gradually for fear of an explosion. It has been chiefly tried in cases of cancer of the uterus, and two cases of this kind are related by Dr. Routh, both of which were cured. In one case, a large cauliflower excrescence was removed by the ecraseur, and a few days afterwards the solution of bromine was applied. She took internally the iodide of arsenic and extract of conium. The report states that she left the hospital with a moveable uterus covered with healthy mucous membrane, and looking herself fat and hearty. The beneficial effects of bromine are not confined to its escharotic action only, but it acts also as a powerful disinfectant. (Dr. C. H. F. Routh, Dr. W. Williams, Dr. Rogers, p. 279.)

Compound Anæsthetics in Midwifery.—There can be no doubt that the administration of chloroform in labour is attended with two great drawbacks, viz., the danger of an overdose, and the retarding effect upon the pains. These are completely avoided by the use of mixed ether, chloroform, and alcohol vapours, which act as a gentle stimulus rather than as a sedative to the labour pains. Mr. Ellis has invented an instrument which is self-supplying with an amount of these vapours, which may be regulated both absolutely and relatively. It may be obtained from Messrs. Savory and Moore. (Mr. R. Ellis, p. 246.)

Division of the Cervix Uteri in Dysmenorrhœa.—Dr. Tilt deprecates the extreme surgical tendency that seems to characterise the present epoch. The utility of dividing the cervix uteri has been much exaggerated. There is so great a tendency on the part of the divided surfaces of the cervix to unite that the operation is generally useless unless followed by dilatation. Dilatation alone is in most cases the safest and best way to relieve dysmenorrhœa and to facilitate conception. Division of the cervix should be restricted to cases where the cervical canal is extremely narrow, or the cervical walls very hard, and to cases wherein dilatation has proved a failure, or where there is flooding from uterine fibroids. (Dr. E. J. Tilt, p. 274.)

Dr. Henry Bennet states that twenty years ago, at Sir James Simpson's instigation, he operated in many cases of narrowed or strictured os uteri, producing symptoms of dysmenorrhœa or sterility. Owing to the frequent relapses, he has long since abandoned this practice, and dilates with very small sponges. Dr. Bennet has never once had an accident with the use of sponges. It is very important not to attempt dilatation if the tissues are at all inflamed. (p. 276.)

It must be remembered that a sphincter muscle exists at the os internum, the circular fibres of which contract immediately the cold uterine sound reaches them, and impede its entrance into the bladder, and a stricture is declared to exist. (Dr. Henry Bennet, p. 276.)

Hysterical Pain.—Hysterical pain is confined to the skin and is always made worse by pinching it up. This is very useful in distinguishing the pain of pleurisy from that hysterical pain which is so common under the mamma. (Dr. C. B. Garrett, p. 284.)

Iodized Cotton in Female Affections.—Iodized cotton is made as follows: Two ounces of iodide of potassium, and one ounce of iodine are dissolved in eight ounces of glycerine, in which solution eight ounces of cotton wool are thoroughly saturated and then carefully dried. A piece of this iodized cotton is most useful as an application to the os uteri in cases of subinvolution with or without congestion or induration of tissue, in cases of chronic inflammatory enlargements and thickenings of the cervix uteri; in cases of pruritus apparently due to acrid secretion passing through the os uteri, and in cases of epithelial cancer of the neck of the uterus. It must be passed up by means of a speculum and pressed firmly against the cervix uteri, over which a piece of cotton wool similarly secured, somewhat larger, and freely saturated in glycerine, should be placed and retained *in situ*, while the speculum is being withdrawn. It may be applied twice or three times a week, each time being withdrawn by means of a thread tied round the middle. (Dr. R. Greenhalgh, p. 375.)

Ovarian Dropsy.—The filling of an ovarian cyst is not the result of mere osmosis, but must be due to the activity of cells on its inner surface, whilst the extension and strengthening of the walls follow. It is probable that were the contents of an ovarian cyst rendered decidedly acid by the injection of dilute acetic acid, that, as in the case of cancer cells, the structure and vitality of the cell lining would be destroyed and their growth arrested, a result which would be followed by resorption of the fluid and shrinking of the walls. (Dr. W. H. Broadbent, p. 381.)

Dr. Junker considers that the injection of acetic acid into an ovarian cyst would prove very dangerous, as the cyst wall becomes quite rotten when the experiment is tried out of the body. (Dr. F. E. Junker, p. 383.)

OVARIOTOMY.—*Treatment of the Pedicle.*—It is well to saturate the piece of pedicle which is strangulated by the clamp, with perchloride of iron. By this means the part is tanned as it were, and become as hard and dry as a piece of leather, and does not cause suppuration about the wound. The use of the clamp is preferable to the plan of leaving the pedicle in the abdominal cavity, for vent is given to a little menstrual fluid every month for two or three times, and this would otherwise enter the abdominal cavity. The clamp is not, however, appropriate in all cases. In some it may be desirable to tie the vessels and leave a short pedicle in the abdominal cavity. It does not prove a successful plan to leave the ligatures long and hanging out of the wound, for they seem to set up inflammation. Silk should be used and cut off short. Silk is preferable to wire, for it is an animal substance, and experiment proves that it may be absorbed. (Mr. T. Spencer, Wells, p. 265.)

New Clamp.—A new clamp has been designed by Mr. Chambers, of the London Surgical Home. It consists of two parallel blades, held together by two racks. In these racks a pinion works, which is moved by the handle. The blades can be connected or disconnected at will, by simply pulling a trigger. It is very simple, and can be applied and removed with great facility. The sides being parallel, this clamp can receive and completely compress a larger pedicle than any other instrument of the kind. It may be obtained from Messrs. Mayer and Co., of Great Portland-street, London. (Mr. T. Chambers, p. 270.)

Application of the Actual Cautery to the Pedicle.—It is preferable in all cases first to apply the actual cautery to the divided pedicle. Should this fail in completely arresting hemorrhage no harm has been done and the ligature may be resorted to without disadvantage. But it is decidedly better to do without ligatures if possible. (Mr. I. B. Brown, p. 269.)

The use of the clamp is now being abandoned in London, by many surgeons, and the division of the pedicle by the actual cautery substituted. The mode of proceeding is to squeeze the pedicle pretty firmly by the clamp and then to divide it by the actual cautery run along it slowly, close to the clamp. The clamp must be removed very slowly, and the cauterized surface be untouched by the finger. Sometimes, though rarely, ligatures must be used in addition, if so, they must be cut off close to the knot. (Mr. Philip Harper, *Lancet, Nov.* 24, 1866, *p.* 578.)

PREMATURE LABOUR.—*New Method of Inducing.*—The plan of simply separating the membranes from a zone or arc of the uterus around the os, does not always induce uterine action, and it is an objectionable thing to leave the catheter in the uterus, as it necessitates the confinement of the patient to bed. Dr. Earle, of Birmingham, introduces, by means of a whalebone guide, a piece of India-rubber tube about two inches long, and closed and tapering somewhat at the upper end. This is left in the uterus, between the membranes and the uterine wall, until pains begin, when it may be removed by means of a piece of string attached to the lower end. Being soft and compressible, it is less likely to injure the uterus, or to rupture the membranes than the catheter is. The patient can walk about retaining this small foreign body without any risk. More than one tube may be passed into the uterus if desired. (Dr. Lumley Earle, p. 241.)

PUERPERAL CONVULSIONS.—*Chloroform.*—In cases of puerperal convulsions, whether we have albumen in the urine or not, the careful, skilful administration of chloroform, even before adoption of the lancet, seldom fails to afford marked relief, and in a majority of cases we may dispense with the excessive venesections of former times; chloroform in such cases, of course, is not to supersede other means usually had recourse to, such as delivery. (Dr. C. Kidd, p. 285.)

STEEL FILLET, A SUBSTITUTE FOR THE FORCEPS IN MIDWIFERY.—Mr. G. R. Sheraton (of Sedgefield, Durham), has invented a steel fillet which is intended to supersede the forceps, vectis, &c., in cases of difficult parturition where the use of these instruments is indicated. It consists of two blades of highly tempered and flexible steel, each nearly the shape of the ordinary forceps blade when viewed sideways. These blades are united by a linked joint at the extremities, and can rotate in their single handle so that they may either be separated to form a loop, or put close together to facilitate their introduction. It is as easy to introduce this instrument as it is to introduce a single blade of the forceps, and when introduced, the blades can be made to diverge and enclose either the occiput or the chin of the fœtus. The narrowness, thinness, and flexibility of the blades renders their application both easy and but little liable to injure the maternal passages or the fœtal head. This instrument can be used without the knowledge of the mother. From the facility with which it can be applied the duration of the operation is shortened. (A wood-cut will be found at page 248.)

Vaginal Speculum.—Dr. Emmet, of the Woman's Hospital, New York, has invented a vaginal speculum, whereby a larger portion of the vagina can be exposed than can be done by any other instrument in use. It will be best understood by reference to the wood-cut at page 255. This instrument does not require the aid of an assistant, as does Sims's speculum, and is much more suitable for long operations on the uterus or vagina. It gives no pain, from the fact that the perineum is compressed to one side of the coccyx. It is made by Ford and Co., of Fulton-street, New York. (p. 254.)

MISCELLANEA.

Actual Cauterisation with Charcoal Pencils.—Pencils may be made of charcoal, nitrate of potash, and a little gum tragacanth, which light and burn like a cigar, and are very useful as an actual cautery. A mass should be made and rolled into cylinders about four inches in length, and of the diameter of a common pencil. (M. Bretonneau, p. 362.)

Chloroætherine.—Chloroætherine is prepared by mixing one part of rectified Eau de Cologne (i.e., Eau de Cologne distilled from rectified spirit) with two parts of chloroform. The result is a clear liquid of specific gravity 1·152. It may be used in cases of labour when it is wished to give something that will not produce unconsciousness but will obviate or lessen the pain of parturition. It may be used in numerous cases of tooth extraction, painful vaginal and other examinations, uterine and urethral stricture. In the latter it is extremely beneficial, as it at once allays the nervous spasm, often extreme, which is always induced at the approach of an instrument in such cases. (Mr. I. B. Brown, Jun., *British Medical Journal, July* 21, 1866, *p.* 88.)

EMPYÆMA.—*Drainage Tubes.*—An interesting case is recorded by Mr. Berkeley Hill, of University College Hospital, in which an empyæma was tapped and a straight pewter catheter introduced as a drainage tube. The advantage of this was that the position of the beak could be altered if the fluid stopped running. 34 oz. of putrid matter were got away the first day, and on the three next days respectively 31 oz., 21 oz., and 21 oz. The next tapping withdrew 16 oz. of pus, perfectly free from odour. The case is related to illustrate the great necessity of regularly withdrawing the fluid from the chest in all cases of empyæma; moreover, that tapping and closure of the orifice, or even leaving the fluid to drain away itself is not sufficient. (*Lancet, Nov.* 24, 1866, *p.* 574.)

NARCEINE.—M. Linné, in some researches on the action of narciene published in the *Journal de Chimie Médicale* states that that alkaloid is more powerfully narcotic than either morphia or codeia. It does not cause nausea, and has not any constipating effect on the bowels. It has the peculiar effect of suppressing the flow of urine. (p. 284.)

NATURAL QUININE (*or Quinoidin*).—In experimenting upon the absorption of quinine by the tissues and its elimination subsequently, Dr. Bence Jones has made the unexpected discovery, that a substance exists naturally in every tissue of the bodies both of man and animals undistinguishable by chemical tests from quinine. It differs slightly from quinine in that its ray in the solar spectrum is a little more greenish. This substance may be separated from the tissues, but it has not yet been crystallized or obtained in sufficient quantity for analysis. A hopeful prospect of possible discovery arises from this as to the explanation of the cause and cure of ague. Assume that a substance like quinine exists, during health, in the textures, can its rapid destruction and removal through the action of marsh miasm give rise to ague? (Dr. Bence Jones, *Medical Times and Gazette, Aug.* 18, 1866, *p.* 163.)

PSEUDO-MEMBRANOUS BRONCHITIS.—This is a very rare form of bronchitis in which the inflammation commences in the smaller tubes, and is attended with effusion of plastic lymph. The inflammation spreads upwards towards the trachea, instead of originating in it and passing downwards as in croup. Pathologically the disease denotes a peculiar modification, without necessarily great intensity, of the inflammatory process. The symptoms are cough and dyspnœa, with expectoration of false membrane at intervals varying greatly in different cases; days, weeks, months, and sometimes years intervening.

The disease is generally rapid in its career, and highly dangerous, although the expectoration of false membrane may be followed by relief more or less complete. (Dr. Austin Flint *on the Respiratory Organs, p.* 337.)

PURIFICATION OF WATER CONTAINING OXIDIZABLE ORGANIC MATTER.—There are three ways of purifying water from oxidizable organic matter. The first is the admixture of permanganate of potash with the water so long as the colour of the permanganate is destroyed. The second is the use of animal charcoal filters or purifiers at the water-works. (The employment of this by a water company would be neither formidable nor expensive.) The third is the employment of Mr. Spencer's "magnetic carbide,"— a form of oxide of iron prepared by him. This substance is stated by him to cause the oxidation of all the organic matter in water without losing its power by use. It is equally applicable, we believe, to the filter-beds of a water-works and to the house filterer. The action of this carbide of iron can be tested readily by means of a solution of permanganate of potash, and it will be found to act as well in a month as at first. In this material, then, lies a means of purifying water at a trifling cost at the water-works. (p. 393.)

Purification of Water by refuse Paraffine oil shale.—It appears that the solid refuse of shale used in the manufacture of paraffine oil, is a most perfect purifier of the filthiest water. Some of it thrown into water highly contaminated by sewage rendered it perfectly clear and fit for drinking purposes. (*Lancet, Sep.* 29, 1866, *p.* 364.)

TOOTH CEMENT.—Prepare a light oxide of zinc by moistening the ordinary oxide with nitric acid, and then igniting it. Thus prepared, it is made into a soft paste with a solution of chloride of zinc, having a specific gravity of 1·9 or 2·0. This soft mass speedily acquires great hardness, which it permanently preserves. It may be coloured grey, if desired, with the least trace of carbon. (Herr Stehle, p. 295.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

1.—ON THE USE OF THE THERMOMETER IN FEVER.

By Dr. John Southey Warter.

[The actual temperature of the body cannot be estimated by the hand, the skin sometimes feeling hot when the temperature is normal, and sometimes feeling cold when the temperature is considerably above the average. In typhoid fever the hand of the patient is frequently cold, whilst the temperature of the body is too high as marked by the thermometer in the axilla. In many diseases the pulse and temperature rise and fall together, so that from the observation of one we may often infer the other correctly; this, however, is not always the case, as in typhoid fever, for instance, the pulse ranges low at first while the temperature is high. In some cases of weakness or nervousness the pulse may be very frequent whilst the temperature is normal. In many cases recovering from acute disease the temperature will fall down for a time below the standard of health, and then again mount up slowly to the normal.]

As far as I have noticed in typhoid fever, as a general rule, the temperature of the body rises high in the first week, while the pulse remains comparatively low, and this is not the case in typhus or in many of those other diseases for which this fever is apt to be mistaken in its early stage. Thus, in typhoid fever I have only one or two records of a pulse up to the eighth day higher than 120, and this number is uncommon, most of them ranging from 90 to 110 beats per minute; in all these cases, however, the temperature was as high as from 102°·4 to 104°·3 Fahrenheit. Exceptional cases to this rule occur, as might have been expected, in young children, on account of the normal rapidity at which the pulse beats at this age.

In typhus fever running its usual course there seems a direct ratio between the rapidity of the circulation and the heat of the body; the pulse and temperature rising together till the fever reaches its climax, when the pulse ranges generally from 110 to 144, and the temperature from 103° to 104°·5. At one time I used to think that a low pulse and high temperature at the commencement of fever always indicated that it was specifically typhoid, but more extended observation has taught me that there are many exceptions to this rule; thus occasionally in febricula, acute rheumatism, tonsillitis, exhaustion from suckling, and phthisis pulmonalis, a temperature of 102° or 103° may occur with a low pulse rate; and these exceptions are the more worthy of our attention as the aspect is in them often typically that of the early stage of typhoid fever; thus in all there may be a clear, white, and perspiring skin, red lips, tinted cheeks, and dilated pupils, and hence without some closer investigation a superficial observer may be often easily deceived in his diagnosis. Perhaps the cases of enteric fever in which the diagnostic use of the thermometer is most striking, are those in which malaise and perhaps some diarrhœa have existed for several days and yet the patient has got about; one day at length, however, the doctor is consulted, and finding the tongue clean and the pulse quiet he is thrown off his guard, and is about to think but little of the attack; wishing to make sure, however, ere he dismisses his patient, he places his thermometer in the axilla, and the mercury rising to 102° or 104° shows him that it is typhoid fever, and not a simple diarrhœa that he has to deal with. Again, in how many cases of supposed typhus does the thermometer, by marking a low temperature, contradict every other symptom! There may be a history of exposure to infection, a rigor may have occurred, the face may be muddy, the eyes injected, the pupils small, the pulse rapid, and the tongue thickly furred; still, if with all this the temperature at mid-day is below 99°·5 or 100°, typhus fever, at any rate, may be stated positively not to exist. Of course in saying this I do not mean to include those cases in which the rash is out and the patient is collapsed, or those cases in which the fever is really past but the black tongue and rapid pulse of exhaustion exist; these tell their own tale and require no such instrument as the thermometer to aid in their diagnosis.

In a fair proportion of cases the acme of typhus is reached on the eighth day, and it may occur, as a rare exception, as late as the tenth, twelfth, or possibly the thirteenth. There seems a decided tendency in many cases of typhus for a rise of temperature to take place on alternate days, and especially on the odd ones; of these a rise on the thirteenth day is most common, and after this a rise on the eleventh. The fever of typhus ends usually by lysis (gradually) about the fifteenth, sixteenth, or seventeenth day, but it may end suddenly in any case by crisis a day or so after the highest temperature has been reached.

In typhoid the highest afternoon temperatures occur probably as early as about the fourth day. There is a tendency to rise on alternate days, especially on the thirteenth, and the ninth, eleventh, and fifteenth; but it runs a course longer than typhus, ending generally some time from the twentieth to the thirtieth day. High temperatures may be occasionally reached at any time during its course, and its decline is gradual, and, as far as my experience goes, seldom ends by crisis.

As regards prognosis in typhus and typhoid fever, the most important rules which can be derived from the records of a single daily temperature seem to me to be two; namely, that a sudden rise of temperature in either indicates some intercurrent disease, as congestion of the lungs, and that a sudden fall of the thermometer in typhoid fever indicates either diarrhœa or hemorrhage.

In treating fevers, at any rate towards their terminations, I think we may derive some hints from the use of the thermometer, for by its marking a normal temperature when perhaps the pulse is frequent, the patient constantly delirious, and the whole mouth black with sordes, it shows us plainly that these symptoms are not the result of cerebral inflammation, but depend in all probability on exhaustion; hence nutrients and stimuli appear the rational curative agents, versus leeches, cupping, and other lowering remedies. Counter-irritation behind the neck may, however, be employed with advantage in these cases; the rationale of its action is not very apparent, but I suspect that in some way by reflex action it increases the tone of the cerebral arteries, and that thus again a healthy balance is restored between these vessels and the tissue which they supply.

The study of thermometry in typhus and typhoid fever, puts, I think, an end to the question as to whether these two depend on separate poisons, or are really one and the same disease. In typhus the pulse and temperature rise more or less together until the seventh, eighth, or ninth day, when they are at their highest, then they begin to decline, and drop down steadily day by day in unison, both becoming normal about the fifteenth or sixteenth day. In typhoid fever, however, all is chaos; the pulse and temperature rise and fall irregularly, taking separate and distinct courses. Both, however, remain, on the whole, moderately high from about the sixth to the fifteenth day, then a fall takes place pretty rapidly, and between this and the twentieth day the normal mid-day temperature is again reached in most cases.

The range of temperature in measles is a short and rapid one, thus contrasting well with that of typhus fever for which this disease is apt to be mistaken. As far as I have observed, it rises to its highest point about the fourth, fifth, or sixth day, and then again sinking rapidly, it falls to the temperature of health about the seventh or eighth. Thus, after the sixth day, at any rate, its course is diametrically opposite to that of typhus; for while in measles after that date both the pulse and temperature are falling rapidly, in typhus fever they are both rising, and on the seventh or eighth day, when the temperature of a case of measles is normal, the temperature of typhus is at its greatest height, ranging from 103° to 105° Fahrenheit. The highest temperature reached in measles is not very great, being rarely higher than 103°; the disease ends usually by rapid lysis, the pulse and temperature falling together. In the curve which is given to represent measles, it is shown how, after reaching its acme of temperature comparatively early, the thermometric wave takes a rapid downward course, being thus entirely distinct from the one representing typhus fever. It may be well to give a caution here as to the fallacy of intercurrent lung disease setting in, for pneumonic congestion of the lung occurring about the fourth or fifth day in measles will keep up the patient's temperature, and make the thermometrical range, taken alone, simulate typhus. As, however, in such cases the lungs would naturally be auscultated, this fallacy is not likely to interfere with actual practice.

In scarlet fever as a rule, the temperature rises to its greatest height from the second to the fifth day, the pulse also being highest at this early date and by its extreme rapidity often aiding in diagnosis; from this point the temperature declines gradually, attaining to the normal about the tenth day. From the study of those cases which I have taken, I imagine that the temperature of a fairly severe case on the second or third day would usually range as high as 104; in mild cases, however, the temperature need not be elevated to any great extent, and I have taken one on the third day as low as 99 Fahrenheit.—*St. Bartholomew's Hospital Reports, Vol. II*, 1866, *p.* 64.

2.—ON THE PRACTICAL VALUE OF ACCURATE DAILY OBSERVATIONS OF THE TEMPERATURE OF THE BODY IN ACUTE DISEASE.

By Dr. Thomas Armetirding Compton, B.A. Dublin.

[Dr. Compton's object in taking up the study of temperature has been mainly with a view to test their practical value for general use in ordinary practice, and consequently his observations have (with some few exceptions) been taken only once daily, but always as near to the same hour as possible (generally about 2 p.m.) The observations have been without exception made in the axilla. The total number of cases in which the temperature and general symptoms have been watched and recorded daily throughout their course amount to 200, of which sixty were typhus, thirty typhoid, twenty pneumonia, fifteen scarlet fever, and the remaining seventy-five comprise cases of febricula, acute rheumatism, erysipelas, chorea, acute tuberculosis, &c. The total number of observations made in these cases amounts to 5000. Dr. Compton considers that a temperature of 98·4 Fah. is too high an average temperature. He considers the healthy range to be somewhere between 95·5 and 98·5 Fah., the most common temperature met with, being probably 97·4 Fah. He says:]

The following are the propositions I wish to establish:—

1st. That a continued daily temperature of 99° Fah., and upwards, indicates an unhealthy condition, and occurs in every case of acute disease. As I have never met with one case in which such a temperature was present, under normal conditions, in a healthy adult, and as every case of the 200 taken exhibits this state of temperature, the proposition may be considered to be proved.

2nd. That any one observation of a very high temperature (such as 105° Fah.), in any case in which the general symptoms do not appear of any particular severity, should lead to a very attentive re-examination, and suggest a very careful watching, especially if occurring in a non-diagnosed case; such a temperature being present only in severe forms of any disease.

In support of this I will mention the following case:—

W. B., aged 29, a well nourished man, of rather sallow complexion, somewhat excited manner, having an occasional spasmodic way of speaking, complained, on admission, of sore throat, and on examination, both tonsils and the back of the pharynx were seen to be considerably inflamed. He gave an imperfect history of having been ill about a week. This, at first sight, seemed to be merely a somewhat severe case of tonsillitis, and no further very minute examination, by the physician in charge, appeared to be immediately necessary. The case was therefore left for the history, &c., to be made out by the clinical clerk, before the visit next day, as is usual in such cases. Finding, however, the man's temperature to be 105·4 Fah., I was interested in the case; and I made out, on further examination, the existence of partial trismus, and that the tongue, when protruded, deviated slightly towards the right side. Having mentioned this state of things to the physician, he returned again on purpose to see the case, when he found the symptoms still more severe, and considered the prognosis very unfavourable.

This man, moreover, died on the next day, some twelve hours after admission, the case being one of septicemia.

Another equally important case of cerebro-spinal meningitis, in a girl of 18, was thought by many who saw it on admission, to be one of hysteria; but here a temperature of 103·5 Fah. was quite sufficient to negative such an idea.

This case also terminated fatally.

3rd. That the thermometer is of great use, as a means of diagnosis in those cases, which frequently present themselves, of general *malaise*, often accompanied by a history of rigors, loss of sleep, &c.; such symptoms being due either to the commencement of one of the specific fevers, or merely to some gastric or uterine disturbance of a temporary character.

In these cases it is often at first impossible to decide, judging only from the pulse and general symptoms; but if the state of the patient be due to the more serious cause, we shall invariably find an abnormal temperature; and on the other hand if due to any other, a normal temperature will be often met with, frequently accompanied by a very rapid pulse. I have never met with one case in which, the temperature being normal, specific fever afterwards developed itself, and although I have nearly twenty examples of this proposition, I will only bring forward one:

E. J., aged 24, a nurse in a ward in which there were several cases of typhus and typhoid fevers, is a well nourished and generally healthy woman. The cheeks are flushed, conjunctiva slightly suffused, tongue covered with thin fur, and inclined to dry: bowels have been loose the last three days, and were open five times yesterday. She complains of frontal headache, pains in all her limbs, want of sleep, loss of appetite, &c. Has not felt quite well for about a week, but continued at her work until yesterday. She "felt very cold all over" three days ago, and sat over the fire, but could not get warm. Her pulse is 72, temperature, 97° Fah. The next day, and the day after, she exhibited much the same symptoms; her pulse, however, going up to 120 and 100 on the two days respectively, whilst the temperature remained steady between 96° and 97° Fah. Two days afterwards she was quite convalescent, and about the ward again. This case bore as strong a resemblance to that of an early stage of typhoid fever as it was possible to do, and for the first few days many considered it to be one of this disesse.

A point of great interest in connexion with this case was the fact that some three months afterwards the patient had well marked typhus, with a temperature of 103° Fah., and a pulse of 120 on the fifth day, which was the first time I had an opportunity of seeing her on this occasion.

4th. That the temperature in every disease has a tendency to run a peculiar course, and has a certain range of altitude, a knowledge of which course and range is of great value as an assistance to us in prognosis.

In connexion with this proposition I should observe that I have not found the temperature in acute disease to be perceptibly affected by the season of the year at which the disease may have occurred, although the temperature of the wards during the year has varied to a considerable extent.

Again, if cases of similar severity at different ages be compared, the altitude attained by the temperature in any disease does not appear to be influenced by the age of the patient, although normally is generally reached somewhat earlier in children than adults.

I have drawn up some charts to show the course of temperature as observed by myself in typhus, typhoid, and scarlet fevers, pneumonia, acute rheumatism, erysipelas, and tonsillitis.

These diagrams are not founded on any average, but merely represent actual typical cases. I have preferred to adopt this plan on account of the fallacies likely to exist in such tables if drawn up from averages. Errors in establishing the exact day of the disease are, in many cases, unavoidable; but these, although only to the extent of a single day, might considerably affect the ultimate result in any general average.

Having discussed typhus and typhoid at some length in another paper I shall not further allude to them. To the other diseases (the course of temperature in which I have endeavoured to illustrate by diagrams) I will briefly refer.

In pneumonia we have high temperatures as early as the second day, with the maximum generally on the third or fourth, and a sudden fall (of perhaps 5° Fah.) to normal on the seventh or eighth.

In acute rheumatism we have a long-continued slightly abnormal temperature, generally ranging between 99° and 102° Fah., and very rarely reaching 103° Fah.

Erysipelas is characterized by very sudden changes of temperature: thus, we often get an alteration of 4° or 5° Fah. in twenty-four hours, and occasionally a fall of 7° or 8° Fah. in the same period. I have observed in several cases that a considerable fall has taken place immediately on the appearance of the characteristic redness.

The temperature of tonsillitis is especially interesting when compared with that of scarlet fever; and as the diagnosis in the early stage between the two is often doubtful, we may frequently be much assisted by the observation of the temperature.

In tonsillitis the temperature in the middle of the day, when at its maximum, which is generally attained about the fourth day, rarely exceeds 100°·5 Fah.; and this occurs with a pulse often below 100. Normal is reached about the sixth or seventh days.

In scarlet fever, on the other hand, the maximum is arrived at about the third day, when the temperature is generally 104° Fah., whilst the pulse is considerably above 100. The normal is not usually attained until the tenth or twelth days.

Although I have taken a few cases of many other diseases besides those just mentioned, yet I have preferred at the present time, to confine myself to the examination of the course of temperature in these seven only, considering that my experience of this subject in any other disease has not at present been sufficient for me to judge fairly of it. I would, however, just allude to the possibility of temperature being of great assistance to us as a means of diagnosis between typhus fever with severe cerebral disturbance (when occurring without any rash), and purely cerebral cases of meningitis or cerebritis. I have only had an opportunity of taking the temperature of two purely cerebral cases, but in both of these the temperature rarely exceeded 101° Fah., whereas with similar symptoms due to typhus fever, we might have expected the thermometer to stand some 3° higher.

I should also state that the highest temperature I have ever taken was 107°·2 Fah., and occurred in a case of pneumonia following tracheotomy; the next highest was 106°·2 Fah., in erysipelas; generally, however, temperatures above 105° Fah. have been rarely met with.

I will conclude this part of the subject by bringing forward a case to prove the value of a knowledge of the course and range of temperature in different diseases.

A boy of fifteen was admitted with general *malaise* and sore throat. His tonsils and the back of his pharynx were somewhat inflamed, but his tongue was moist and nearly clean.

After the first few days he complained of nothing except weakness, although his voice continued very husky. His appetite remained pretty good throughout, and he slept well. About the fifteenth day he had some bronchitis, and on the sixteenth some hemorrhage, with a loose motion; previously his bowels had been somewhat confined. The hemorrhage did not recur. There was never any rash or abdominal tenderness, and the physician in charge of the ward considered the patient's illness to be due to the apparent symptoms manifested, viz., the sore throat, with probably some laryngeal mischief, and the thoracic complication.

The course of temperature was that of typhoid fever throughout; and at the time I mentioned this fact to several, but little expected to be afforded any corroboration of my statement, as the lad's temperature fell to normal on the twenty-eighth day; and two days after this he was up and about the ward.

However, as the case was not considered to have been one of typhoid fever, there was no especial necessity to be careful with his diet, and he was placed on meat diet. Having a very hearty appetite he did good justice to the food allowed him, besides partaking of some extras which were surreptitiously conveyed to him by his friends. The consequence was, to use his own expression, he went to bed "feeling very well," but "very full." Some hours afterwards symptoms of colic came on, which not being relieved by the appropriate measures adopted, were followed by those of peritonitis of which the lad died on the seventh day from his seizure.

The *post mortem* disclosed a quantity of purulent fluid in the peritoneum, and numerous ulcers of Peyer's glands, the majority of which had already cicatrized but in many the healing process was still going on. The peritonitis had been caused by an overloaded intestine, previously weakened, the colon being completely stuffed with scybala. There was no perforation.

5th. From the last proposition it follows, that the same altitude of the thermometer attained at one period of any disease is not of the same importance as the same height reached at another time in the same disease.

Thus, in typhoid fever, a temperature which has been rising for two or three days, reaches perhaps 104 Fah., between the seventh and fourteenth days, without causing any anxiety; whereas, should the same phenomenon occur about the twenty-eighth day, a fatal termination may probably be expected.

And again, the actual altitude attained on a certain day in one disease is not of the same importance to our prognosis as the same height reached on the same day in another disease. Thus, a temperature of 104° Fah. in erysipelas is very common during the first week, and need not give rise to any alarm; but should such occur at the same date in acute rheumatism I should consider it of much more importance.

6th. That although, in all diseases, a high range of temperature generally indicates a severe case, with a slow convalescence, and a low range usually occurs in a mild case, and is followed by a rapid convalescence; yet there is no actual temperature in any disease which necessarily fortells a fatal termination. Thus, I have registered 105°·6 Fah. in a severe case of typhus ending favourably, 106°·3 Fah. in erysipelas, 105°·3 in typhoid; and each of these temperatures was the highest I ever took in the respective diseases.

I certainly once registered a temperature of 107°·2 Fah., in a fatal case of pneumonia; but the great majority of fatal cases generally, although always exhibiting abnormal temperatures prior to collapse, have by no means had any extraordinarily high ranges. I believe that an abnormal course of temperature is more often the precursor of a fatal termination than any universally high range.

7th. That in the majority of cases a rise of temperature is contemporary with a rise of pulse, although such is often not a proportional one, and may not take place at all unless the alteration in temperature be as much as 1½° or 2° Fah.

Cases of typhus are, perhaps, the best examples of the simultaneous alteration of pulse and temperature.

8th. That where the temperature and pulse together do not coincide with the general symptoms, the two former may be generally relied on as to the actual state.

Thus, in typhus, towards its close, we often get a continuous fall of temperature and pulse for perhaps three or four days before there is any improvement in the general symptoms ; and in these cases the two former always foretell rightly.

There is, however a well marked exception to this rule in the later stages of typhoid fever, as mentioned in another paper.

9th. That where the temperature and general symptoms agree together, but do not coincide with the state of the pulse, the two former may generally be relied on as to the actual state.

Thus, in cases of hysteria, and in cases of excitable persons approaching convalescence after fever, we often suddenly get a very high pulse, which is sometimes continuous for days, although the temperature and general symptoms are neither of them adverse.

10th. That in those cases in which the pulse and general symptoms remain the same, a moderate fall of temperature on one occasion is not to be relied on ; but should such a fall continue in a moderate and gradual manner, for some days, and at such a period when a fall was to be expected, the temperature may then be depended upon. Severe cases of typhus, towards their close, often give examples of this sort.

In those cases, however, in which the pulse continues frequent, and the general symptoms are severe and without improvement, a considerable fall of temperature (say 5° or 6° Fah.) is to be regarded with anxiety, being probably due to some internal hemorrhage, or to the commencement of a state of collapse.

Thus, in a case of icterus, in a girl of 15, due to obstruction of the common duct by a hydatid cyst, and accompanied by peritonitis, on the three days preceding death, the pulse had stood at 140, 140, and 136 respectively, the temperature having been on the same days 104°·2, 104°·5 and 99° Fah. ; whilst the general symptoms continued without alteration. Here, on the last day, we got a sudden fall of 5½° Fah., which was the first sign of collapse terminating in death some twenty-four hours afterwards.

Again, in a case of typhoid fever, on the seventeenth day there was a sudden fall of 4° Fah. from the preceding day in consequence of considerable internal hemorrhage.

11th. That in those cases in which the pulse and general symptoms continue the same, being the one frequent and the other severe, a continuous rise of temperature for some days, occurring at a period of disease at which some improvement might generally be expected, is usually the precursor of a fatal termination.

Thus, in a case of typhoid fever, terminating fatally on the thirty-sixth day, the pulse and general symptoms continued without change until the thirty-fifth day; the former having stood at 92 on the twenty-ninth day, remained steady and rather below that frequency until the thirty-fifth day, when it suddenly rose to 124. The temperature, however, rose continuously from the twenty-ninth day nearly a degree a day, until it stood 5° higher on the thirty-fifth than on the twenty-ninth day —the height registered on the thirty-fifth day being 104° Fah.

12th. That although it is possible that the state of the temperature alone in acute disease may, perhaps, hereafter prove to be the one safest symptom to rely upon if taken by itself (and I believe it is at present, at least equal to the state of the pulse, and of greater value than this certainly, if only its frequency be taken into account), yet the temperature must be considered merely as an aid, and all other symptoms must be carefully examined into, as it is on comparison with these that its greatest value is always to be found.

The preceding propositions, which are not intended to be put forward as dogmatic rules, being merely the result of a very limited experience, and being probably only approximations to the real truth, which is always deep down "at the bottom of a well," imperfect though they be, nevertheless contain, I think, amply sufficient to prove the great value of the daily observation of the temperature in acute disease.

I will now refer to those who disregard the value of the clinical thermometer, and amongst these are many of high authority, whose opinion is entitled to great consideration.

What, then, is the reason of the disregard of such? 1st. Some do so because they allege that they can sufficiently distinguish by their hands normal and abnormal states of temperature, and can carry such sensations in their minds, or commit them to paper under such expressisons as "cool," "moderately cool," "warm," "hot," "very hot," "pungently hot," &c.

The fallacy of these expressions can, however, be easily ascertained by any one who will take the trouble, in a few acute cases, first to write down his sensations, and then take the temperature by a thermometer.

Heat and cold are only temperature sensations, and as our hands vary considerably in their temperature, it follows that a body submitted to a hand, the temperature of which is 95°, will refer to the mind a very different sensation to that which it would do were the temperature of the hand 10° or 20° lower.

As an example of this fallacy, I will only mention the following fact :—

I have heard a physician of high standing observe that the skin of a patient was "moderately cool," when the actual temperature in the axilla was 104° Fah. This is by no means an uncommon occurrence. 2nd. There are some authorities who state that an abnormal temperature generally does not occur at all in cases of acute fever, and on this account undervalue the thermometer.

I can, however, honestly state that I have never met with one such case, neither have I found any one who has used a thermometer who has done so, except the patient were in a state of collapse just prior to death.

Consequently, with all due deference to such authorities and their statements, I am strongly inclined to think that the actual existence of such cases is more than doubtful ; and if the temperature of a patient which is really 104° Fah. can be called "moderately cool," it will not be difficult to understand how such cases have been supposed to occur.

3rd. There are others, again, who disregard the value of the thermometer because they think they have tried it and found it wanting.

These have tried it certainly, according to their own manner, but generally with utterly wrong and probably self-conceived ideas as to its use. They have started on their trial with the belief that the abstract degree of heat was all that they had to consider. Such as these, consequently, soon find out that a thermometer will lead them into all sorts of blunders, and properly discard it. But would these be merely content with considering the height of frequency to which the pulse had attained, without taking into account its volume, regularity, its condition yesterday, the disease and the individual in whom it occurred ?

Well, then, some very similar conditions must also be considered before placing an actual value on any one observation of temperature—thus, the disease, its period, its height yesterday, &c. ; and without reflection on these points, any observation will be useless indeed.

It would, no doubt, save a great deal of trouble if we could decide our prognosis and diagnosis by means of the temperature alone, and also without any practice or experience in the use of the clinical thermometer ; but would any one discard the stethoscope merely because some experience in its use was required before any one could be in a position to place a value on the sounds he might hear, or because, even after considerable practice, the value of some sounds (such as certain cardiac murmurs) still remain doubtful?

In the same manner, a person must have some amount of practice with the thermometer before he is in a fit position to put an actual value on any one observation; and any error in diagnosis or prognosis which a beginner may make will be due probably to this wrong interpretation of a certain fact.

The greater his experience the less will be the chance of such an error, and the more valuable will his thermometer be to him.

I will just give two examples of numerous mistakes liable to be made by this class of objectors.

A case of febricula presents itself, and a very high temperature is found within a few hours of the first symptom of *malaise.* The observer in question necessarily thinks that his patient must be suffering from a very severe form of fever, probably typhus, and is proportionally anxious, but in forty-eight hours perhaps, to his delight and astonishment, the temperature is again normal, and the patient convalescent. Such an observer, having considered only the abstract degree of heat, immediately discards his thermometer, although his error of diagnosis and prognosis was due to the wrong interpretation of a fact. Had his experience been greater, the very fact which so alarmed him would have had, probably, an exactly opposite effect, for he would have known that the disease could not at any rate be typhus fever.

Again, to take an opposite example.

In a case of typhoid fever, in which the general symptoms are severe, the temperature, which has been fluctuating about 103° Fah., suddenly drops 4° or 5° without any improvement in the general condition of the patient.

Such observers, considering only the abstract degree of heat, regard the altered state of temperature as a favourable sign, and are surprised at, very possibly, finding their patient dead on their next visit. Had they had more experience, they would have known that such a fall was a very unfavourable symptom under the circumstances, being probably caused by intestinal hemorrhage, or else being the commencement of a state of collapse; as in typhoid fever, the general symptoms improve considerably before the temperature reaches normal. All the blame of such unfortunate prognoses is, of course, laid on the thermometer, instead of on their own ignorance and rash judgment.

The above are examples of the misuse of the instrument, in the use of which we must always be very careful not to jump at conclusions, but strive honestly to arrive at the proper interpretation of the additional fact which we have just registered; by so doing we shall, by its assistance, reap an abundant reward in a more successful diagnosis—a more confident prognosis.

In conclusion, I am convinced that the study of the thermometry of disease is as yet in its infancy; and believing that such a study, instead of teaching any disregard for the pulse, or other important symptoms (as some have wrongly supposed it was intended to do), will further enhance their value, and generally conduce to a more attentive examination of each symptom manifested in acute disease, I consider the subject to be one well worthy the attention of every member of our noble profession, and one which will amply repay the few extra moments spent at the bedside.—*Dublin Quarterly Journal, Aug.* 1866, *p.* 60.

3.—A CONSIDERATION OF THE COURSE OF TEMPERATURE AND PULSE IN TYPHUS AND TYPHOID FEVERS, AND SOME OF THE POINTS OF DIFFERENCE BETWEEN THESE TWO FEVERS SHOWN THEREBY.

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[The cases from which the following data were ascertained occurred in St. Bartholomew's Hospital during the last two years, and daily observations were taken of them. The number of cases amounted to sixty of typhus and thirty of typhoid.]

Typhus, as met with under the above-mentioned circumstances, seems to occur generally in two forms:—1st. The milder form, the subjects of which are children, or previously healthy adults under thirty, who have led sober and regular lives; 2nd. The more severe form, which has been the one somewhat the more frequently met with. This has occurred either in persons of a more advanced age or in younger adults of unsound condition from previous disease, or in consequence of intemperate habits.

In both of these forms the temperature rises in a more or less regular manner to its maximum, when it falls continuously about a degree a-day until the normal is reached; and although having further fallen below this, it may rise to regain its ordinary height, it will not again exceed this point unless some complication be at hand.

In all cases, also, of either form there is a very uniform temperature of about 104° Fah. on the seventh day; with this exception there is no temperature peculiar to any one day.

In the milder form of the disease the pupils are seldom contracted; the tongue, although generally thickly coated, never exhibits the dry, brownish-black fur of the more severe type; subsultus is not often, floccitatio very rarely present; the delirium (which occurs only at night) is slight, and the rash is not usually very copious. In such the general range of temperature is comparatively low, the termination of the case is, without exception, favourable; and the convalescence, which may be said to begin about the fourteenth day, is rapid.

The temperature attains its maximum on the seventh or eighth days, the average maximum being 103°·7 Fah., the highest registered 104°·3 and lowest maximum 102°. The normal is reached on the twelfth or thirteenth days.

In the more severe form we often find contracted pupils, a dry, brownish-black tongue, with sordes on lips and teeth, great subsultus and muscular tremors, often floccitatio, delirium frequently present by day as well as night; and towards the termination of the case, a semi-comatose condition often comes on, which occasionally alternates with severe delirium at night. The rash in these cases is generally copious. Under this form I include those exceptional cases of typhus which are attended by considerable diarrhœa; these occur in about ten per cent. of all cases, and do not in any other respect differ from ordinary severe cases of the fever.

In the severe form, then, the range of temperature is high, the termination not always favourable, and the convalescence, which does not begin until the end of the third week, is very tedious.

In these cases the maximum temperature is attained between the seventh and tenth days (generally on the seventh or eighth), the average maximum is 104°·4 Fah., highest registered 105°·6 Fah., lowest maximum 103°. Normal is reached between the fifteenth and eighteenth days, both inclusive.

High temperatures, such as 105° Fah. and upwards, do not foretell fatal terminations; but high ranges, generally, at the commencement of cases foreshow severe attacks, and are invariably followed by long convalescences. Fatal cases always exhibit abnormal temperatures prior to callapse, but not necessarily very high ones. Judging from the fatal cases I have witnessed, I believe that an abnormal course of temperature much more generally precedes a fatal termination than any unusually high range.

Such cases have appeared to differ from the ordinary run of severe attacks terminating favourably in one or more of the following ways:—

1st. They exhibit a temperature of only 102° or 103° Fah. about the sixth day, which is below that generally met with even in mild cases; and considering the severity of the symptoms we should expect to find a temperature nearly 2° higher at such a date.

2nd. They have a constant temperature of about 102° Fah. for a week or more.

3rd. They exhibit a fall of perhaps 3 or 4° Fah. on some day which, in an ordinary severe case, we should not expect any such fall (say on the tenth day).

4th. They have a temperature of about 104° Fah. as late as the sixteenth day, when even in severe cases the normal ought to be nearly reached.

The pulse generally in typhus rises and falls with the temperature; and between the fourth and ninth days is above 100, often very considerably so; its maximum is reached rather later than the temperature, and normal is attained some two or three days after this point of temperature has been arrived at.

It may, however, especially in persons of nervous temperament, be kept up beyond this date.

In typhus a temperature of 99°·5 Fah. corresponds to a pulse of about 100, a temperature of 100°·5 Fah. with a pulse of 105 and so on; an alteration of five beats of the pulse corresponding to each degree of alteration in the temperature.

In the majority of cases, however, unless the alteration of temperature be upwards of one degree, we shall find, probably, no corresponding increment or decrement in the pulse, and the above only holds good in a general sense.

I have, however, noted down each variation of 2° and upwards in all cases of typhus which I have, in order to ascertain the contemporary behaviour of the pulse; and find that every such variation was attended by a corresponding alteration in the pulse of an average of five beats for each degree. There were only five exceptions to this rule out of the whole number, and in two of these the variation in pulse occurred the next day without any corresponding alteration in temperature.

In concluding these observations on typhus fever I should mention that by far the most frequent complication in these cases was bronchitis, coming on about the twelfth day.

Considerable feebleness of the cardiac systole and impulse was also noticed in several instances.

The fatal cases amounted to about twelve per cent., and occurred either within the first week or else between the twelfth and sixteenth days.

In the former case the deaths seemed to be due to the intensity of the fever poison itself, and in the latter to the circulation in the system of the products of tissue metamorphosis. In only one case, however, were typical uremic convulsions observed. These fatal cases occurred in patients over 40, or in adults above 25 who had led irregular lives. The proportion of male to female deaths in the same number of cases was 7 to 5.

At the *post-mortem* examinations of these cases ulceration of Peyer's glands was never present, and the only peculiar appearance noticed was a dark fluid condition of the blood, and occasionally a dark and abnormally dry condition of the muscles. In one case there was a remarkable effusion of dark fluid blood into the substance of the recti muscles of the abdomen.

The enlarged congested liver and large soft pulpy spleen have been met with both in typhus and typhoid. I will now proceed with the consideration of typhoid fever, which has been met with under two forms of such unequal severity, that I propose for convenience sake to divide the cases as I have done those of typhus, into two classes; one comprising the milder, the other the more severe form.

I should, however, observe that although a severe case of typhus may often be foretold at an early period, yet it is impossible to do so at a similar date in typhoid fever; as I have observed several in which the mildness of the symptoms during the first week or ten days might have augured an early convalescence, eventually turn out the most protracted character.

In typhoid fever the temperature generally rises, after numerous daily fluctuations, to its maximum; and having attained this point exhibits the same characteristic irregular decline, falling perhaps for two or three consecutive days, and then rising again for two or three more, and so on; the fall, however, preponderating over the rise, until the normal is reached, after which daily fluctuations will probably continue for a few days longer. The majority of cases come under the first mentioned form, ultimately proving of moderate severity, and terminate favourably towards the end of the third week.

In these the pupils are slightly dilated; the tongue, although inclined to dry, and generally abnormally red at the tip and edges, often continues nearly clean throughout. A very common form of tongue which I have observed in such cases has a rather dryish patch, in the form of an isosceles triangle (the base of which is at the tip), running up the centre, with two moist strips of thin white fur on either side. Subsultus and tremors in these case are rare, and delirium (as often absent as present) is usually very slight, and comes on during the night only. Intestinal hemorrhage does not occur, and thoracic complications of importance are rare.

In these the temperature attains its maximum between the seventh and eleventh days (but this is not reached more frequently on one day than on another); the average maximum is 103·3 Fah.; highest registered 104°·5, lowest maximum 102°·2 Fah. The normal is arrived at between the fifteenth and twenty-first days.

In the more severe form we generally find that either intestinal hemorrhage occurs during the course of the case, or that we have to deal with a low form of pneumonia, which is a frequent complication, and one which not uncommonly is the immediate cause of death at an advanced period of the fever.

In these protracted cases we get, towards the later stages, dilated pupils, a tongue which is either covered with brownish black fur, and dry, or one which has its central surface glazed, edges and tip red. In both of these transverse furrows may generally be observed. Subsultus and tremors may also be met with, but very rarely floccitatio; delirium, usually of a low form, is pretty constant at night, but rarely present during the day.

In such cases which terminate favourably the temperature reaches its maximum between the nineteenth and twenty-first days, the average maximum is 104°·1 Fah.; highest registered 105·2, lowest maximum 103°8 Fah. The normal is reached usually between the twenty-third and thirty-fifth days.

In typhoid fever a high range within the first fourteen days generally foretells a severe and protracted case, but a low range during this period does not necessarily foreshow a mild case, even if the general symptoms be also favourable.

A sudden fall of several degrees, especially if occurring in a severe case, about the end of the second week, and without improvement in the general symptoms, is to be considered with anxiety, such decline being probably due to intestinal hemorrhage, or to the commencement of a state of collapse.

In such an event the pulse, which very likely up to this time may have kept below 100, usually rises suddenly twenty or thirty beats.

In these cases, when the patient does not immediately succumb, the fall in temperature and rise of pulse are frequently followed, within the next twenty-four hours, by opposite phenomena, the temperature rising higher than the point at which it previously stood whilst the pulse falls considerably.

Again, if we meet with a case in which the general symptoms continue severe, but the temperature, although an abnormal one, fluctuates for some days at a point disproportionally low when compared with the general symptoms, we must consider such low range in an unfavourable light rather than the opposite; especially ought this behaviour of the temperature to be so interpreted if it occur in a case of which the date of the commencement of the fever is unknown or doubtful, and in which the period may then be as late as the end of the third or the fourth week.

Almost without exception in cases which terminate favourably, we find that the general symptoms reach their normal, if I may so speak, several days before the temperature; and although anti all fluctuations of the temperature above "the normal" have ceased, the patient cannot be considered safe from liability to a relapse, and great care must be still taken with his diet; yet we may expect in the majority of cases, that if the general symptoms have thus improved we shall have a favourable issue, notwithstanding the slight fluctuation of the temperature above "the normal."

It is the knowledge of this fact that causes us to look with suspicion on cases in which the normal temperature is apparently about to be arrived at before the improvement in the general symptoms. Fatal cases often exhibit a continuous, although often slight, daily rise in temperature for four or more days previous to collapse, and this may occur without any corresponding alteration in the pulse, or without any decided change in the general symptoms.

As we know that a continuous rise for four or more successive days is not met with generally in cases which end favourably, such a rise should put us on our guard.

In typhoid the pulse, to a considerable extent, fluctuates with the temperature, although the exceptions to this rule are much more frequent than in typhus.

However, generally speaking, an alteration of about five beats of the pulse occurs for each degree of alteration in the temperature. Thus, a temperature of 99°·5 Fah. will correspond to a pulse of 90; a temperature of 100°·5 with a pulse of 95, and so on.

The range of pulses which may be found in different cases, to correspond to the same degree, is, however, very great; thus, between 102 and 103° Fah. I have met with pulses between 80 and 154.

High pulses in typhoid fever do not often occur in adults; and when such are not clearly due to some manifest complication, there seems some reason for believing that tubercular deposit in the lungs or the general state of tuberculosis, is the cause of this phenomenon.

Fatal cases have occurred in 13 per cent of all cases, and the proportion of male to female deaths, in the same number of patients, was as 5 to 4. Death took place on the fifteenth, thirty-fifth, and thirty-sixth days, and also in one case on the sixteenth day, of a relapse; besides these cases, the date of death in another patient could not be ascertained, but it was not earlier than the fifteenth day, and very probably took place at a much more advanced period of the disease.

The cause of death was due either to peritonitis, a low form of pneumonia, or asthenia.

In all there was well marked ulceration of Peyer's glands. A low form of pneumonia was the most common complication, and in several cases the fever commenced with some inflammatory affection of the throat.

In all cases diarrhœa was all but universally present, at some time in the course of the fever, although in many constipation prevailed for the first fortnight.

Considerable hemorrhage occurred in four cases. Relapses took place in 12 per cent. of all cases. I will now conclude by summing up the differences most frequently observed between these two fevers, as far as their pulses and temperatures are concerned.

1st. The range of temperature is somewhat lower in typhoid, in which, however, a continuous abnormal temperature is of much longer duration.

2nd. The defervescence of typhus is regular and continuous, the temperature falling generally about a degree a day, until the normal is reached; whereas in typhoid, the fluctuations from day to day are considerable, and the fall is not a continuous one daily.

3rd. There is a great tendency in typhus to attain certain temperatures, and to reach certain points in its course, on certain fixed days. Thus, in the great majority of all cases, a temperature of about 104° Fah. is met with on the seventh day; in a very large per centage the maximum is reached on the seventh or eighth days; the normal is generally attained, in mild cases, on the twelfth or thirteenth days; and in more severe ones between the fifteenth and eighteenth days.

In *typhoid* there is no particular temperature generally arrived at on any one day; the maximum may be reached at any time between the seventh and twenty-first days; and normal is attained, in mild cases, between the fifteenth and twenty-first days; whilst, in the more severe ones, it may be attained on any day between the twenty-fourth and thirty-fifth.

4th. The pulse is generally less frequent in typhoid; and although in both fevers the pulse usually fluctuates with the temperature (about five beats of pulse corresponding to each degree of alteration in temperature), the frequency of the pulse in the two, for the same degree of altitude, is different.

Thus, with a temperature of 100°·5 Fah., we shall get an average pulse of about 105 in typhus, but of only 95 in typhoid, and so on; at each degree the pulse being about ten beats more frequent in typhus than in typhoid. Moreover we sometimes meet with a case of typhoid fever in which the pulse very slightly exceeds normal throughout, but such a case I have not observed in typhus.

5th. In typhus the temperature reaches normal two or three days before the pulse, and the general symptoms often continue severe several days after the pulse and temperature are both normal. In these cases, however, if the defervescence has been regular, and has occurred about the usual period, the case will terminate favourably, notwithstanding the often very unfavourable condition of the patient.

In typhoid, on the other hand, as I have before mentioned, the temperature keeps up several days after the general symptoms have been considerably improved, and we must look with doubt rather than otherwise, on cases on which this rule appears about to be broken—that is, on cases in which the general symptoms continue severe, although the temperature is but little, if any, above the normal.

6th. Relapses of typhoid have occurred in 12 per cent. of all cases, and in these the temperature ran a similar course, as in the former attack, although at a somewhat lower range.

Relapses of typhus, with a corresponding recurrence of abnormal temperature, have never been met with, although the number of cases of typhus under observation has been double that of typhoid.—*Dublin Quarterly Journal, Aug.* 1866, *p.* 71.

4.—ON THE INFLUENCE OF ALCOHOL ON THE TEMPERATURE OF FEBRILE AND NON-FEBRILE PERSONS.

By Dr. SYDNEY RINGER, Professor of Materia Medica at University College, and Assistant Physician to University College Hospital; and Dr. WALTER RICKARDS.

In this paper the authors give the result of some observations on the influence of alcohol on the temperature of non-febrile and febrile persons, and a few on rabbits. The authors gave alcohol in poisonous doses to three non-febrile adults. The temperature was greatly depressed in two. The depression amounted to 3° Fahr. In the third case the temperature was but little influenced. The subject of this observation was a confirmed drunkard. He confessed to getting drunk whenever he had the opportunity.

Alcohol was also injected into the rectum of two rabbits. In both the temperature was considerably depressed. The depression amounted to 15° Fahr.

The authors therefore conclude that alcohol in poisonous doses causes a very considerable depression of the temperature of the body of non-febrile persons, and also that it is probable that habit obviates this effect. Further proof in favour of this latter conclusion is given in a subsequent part of the paper.

The rapidity of the fall of the temperature after death of two patients and one rabbit was ascertained. This is compared with the rapidity of the fall of the temperature after the use of alcohol. It was found that the temperature falls as rapidly after the use of alcohol in poisonous doses as after death. The circumstances determining the cooling of the body, however, differ in the two conditions. Thus during life much heat is carried off by the air inspired into the lungs. Assuming that the cooling effects of respiration are equal to those that result from radiation from the surface of the body, it follows that alcohol possesses the power to diminish by one-half those processes that produce the heat of the body.

Both of the patients whose temperature was depressed suffered from nausea and vomiting. In order to ascertain if the depression of the temperature were due to the conditions that accompany vomiting, tartar emetic was given to a patient every ten minutes, and continued long after vomiting was produced. The administration of the antimony was continued seven hours. No depression of the temperature resulted. Thus the authors conclude that the depression of the temperature was not due to the vomiting produced by the alcohol.

Alcohol was next given, in ordinary doses, to non-febrile persons. Eleven observations were made. In eight the temperature was depressed. In three cases the temperature was unaffected. These three persons were strong adults. The quantity of alcohol given them was small (an ounce of brandy). Two of them were confessed free drinkers. The amount of depression was slight. The authors thus conclude that alcohol, when taken in ordinary quantities by non-febrile persons, causes a slight depression of the temperature of the body, but that the amount of depression which occurs is too slight to contra-indicate its use.

Numerous observations were made to ascertain the influence of alcohol on the temperature of febrile persons. To some of the patients very large quantities of alcohol were given. To a a child of twelve years old eleven ounces of absolute alcohol were given on one day. From these observations the conclusion is drawn that ordinary and extraordinary quantities of alcohol cause only a slight and temporary depression of the temperature of febrile persons, and consequently alcohol cannot bring the temperature of febrile patients to that of health. But if alcohol should be indicated by the general condition of the patient, it will also to some extent act beneficially in virtue of its power to cause some diminution of the temperature of the body.

Some observations were made to ascertain the influence of alcohol on the pulse. From these the conclusion is drawn that alcohol increases the force of the pulse, but lessens its frequency.

In conducting these observations the following precautions were taken: the patients were kept in bed; all the conditions were kept the same; the thermometer was kept the whole time in the axilla. and the temperature was noted every few minutes. The observations were continued many hours--in some cases during the entire day.—*Lancet, Aug.* 25, 1866, *p.* 208.

5.—A CASE IN WHICH MEASLES AND TYPHOID FEVER WERE COINCIDENT.

By W. B. Kesteven, Esq., Holloway.

On the 14th, 15th, and 16th of last December, a girl, aged 14 years, was ill with measles. A few days afterwards another girl, aged 11 years, was indisposed, and suffering from symptoms of fever of the enteric type. On or about the 30th of December, a few of the rose-coloured spots characteristic of typhoid fever made their appearance, and increased in number, but did not exceed thirty or forty over the whole of the trunk. On the 8th of January, two younger children presented the ordinary symptoms and eruption of measles, and passed through the disease in the usual course.

On the 8th of January I was considerably puzzled by finding my typhoid patient exhibit the general symptoms and special rash of measles in addition to that of the enteric fever. I began to doubt the correctness of my diagnosis as to either the one or the other of the two diseases, and was, therefore, only too glad to avail myself of the superior judgment and greater experience of Dr. Jenner, who, after careful investigation into the history and condition of the patient, confirmed my opinion that I had here a case in which these two eruptive fevers were coincident. At the same time my doubts were more than justified by Dr. Jenner's statement that he had only once before met with such an occurrence. The rarity of such coincidences is, therefore, obvious; but their occasional occurrence has been recorded. Thus Mr. Broke Gallwey published in the Lancet of the 28th of August, 1858, a case of small-pox supervening on measles, and another on scarlatina. In the *Journal of Public Health* for October, 1856, is published a paper of my own, read to the Epidemiological Society, and in which I described a series of cases wherein measles and scarlatina were concurrent. Doubtless other instances of the same kind have happened; but I have notes only of the above.

It may not be uninteresting, with reference to etiology in the above case, to add that exactly opposite the house in which it occurred there is one of the ventilation gratings of the street sewer. I do not doubt that these openings have much to do with the spread of typhoid fever in the suburbs of London. I have been in practice in this place nearly thirty years, and for upwards of twenty years of that period I never met with a case of typhoid fever. Since the new sewers have been made the cases have become very numerous. Doubtless it would be a great benefit to do away with cesspools in crowded districts; but it is doubtful whether, as at present constructed, without ventilation shafts, the sewers are anything better than elongated cesspools, diffusing through their air gratings the causes or abettors of various forms of disease.—*Lancet, June* 9, 1866, *p.* 619.

6.—ON THE CAUSES OF DISEASE.

By F. C. Skey, Esq., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

If you inquire into the recent history of a person suffering from disease or illness, whatever form almost it may assume, you will learn of some event or circumstance of recent occurrence that has drawn more or less suddenly whether on his circulating or nervous system, by which the vital powers of his body have been reduced. This condition is commonly known under the term exhaustion, and is the result of a sudden draught on the powers of the constitution, by which the relations hitherto existing between the physical frame and the powers of life inhabiting it, are for the time deranged.

So long as health is undisturbed by any counteracting agency beyond the natural wear and tear of time, life will be prolonged to its full term of years, but in our path through life we are liable to disturbing influences which destroy the equilibrium. These accidents draw on the vital powers and reduce them below the level of standard health, and disease follows as an almost certain result. Let us suppose a man to consume each day a given amount of food. Accident deprives him of a portion of this food, which I will conclude equal to one-half of a day's ordinary consumption. To this amount he fails in the supply of material of blood making, and his circulation is weakened in proportion. That man has become more susceptible of disease, in a ratio with his loss of blood, than he was prior to the occurrence.

Another man, under circumstances whether of choice or compulsion, sustains an unusually great and prolonged physical effort, whether in walking, or riding, or in rowing, of which we see abundant examples in the annual folly of the University boat race. Inasmuch as the serious consequences of these physical efforts, on which the reputation for learning of the great Universities of England depends, are remote, they are not detected. I have seen several examples of disease developed in men who have undertaken a walking tour in Switzerland, who have overtasked their powers of endurance, have returned home exhausted by the effort, and have been laid up ill for months.

Any cause which tends to reduce the quantity of blood in the system, whether by direct abstraction or by deprivation of food as the source of blood, reduces the vital powers, and disease or illness in some form is the consequence. The same principle is involved in the defective supply of oxygen to the lungs, and indeed is applicable to any and all causes that either reduce the blood in quantity or impair its strength for circulation. Any great and unusual physical effort reduces the powers of life in proportion to its degree, more especially when coupled with loss of sleep. Exposure to cold, when severe or prolonged, is a source of subsequent derangement of health, and among other causes may be included either shock to or exhaustion of the mental powers, such as great anxiety of mind or mental emotion, and laborious and continued mental labour. Of the above causes of disease, direct or indirect loss of blood and deficient food are the most common; and although I have above referred to them, as also to the remaining causes, as sudden, they may be more or less chronic both in their nature and in their consequences. I referred rather to sudden and severe illness or disease as the result of some sudden and palpable change in the routine of life of the individual, in which the evidence of cause and effect is conclusive. I believe, then, that in the large majority of illnesses you may trace back the malady, be it what it may, to one or other of the above incidents as its direct and obvious cause; and as certainly as one or the other of these causes has existed, you may with reason and probability look out for its result in some early attack of illness, medical or surgical, local or constitutional. Whatever function or structure is most liable to derangement, the consequences fall upon it in the first place. It may be diarrhœa, or pneumonia, or abscess, or erysipelas, or rheumatism, or general debility. If you will adopt the habit of tracing back the recent history of any or every patient you prescribe for labouring under disease, you will learn something bearing upon it of the nature I have referred to, some effort overtaxing the physical powers or reducing the circulation or nervous powers of the system; and if so is it not a reasonable inference that all disease begins by a stage of *weakness?* The first link in this chain consists in a reduction of vital power, from which reduction the effort of the physician or surgeon consists in restoring the lost power and bringing the patient back to the standard of health. In illustration of these principles I will relate a case that occurred to me recently. 'A healthy lady, of about 34, consulted me on account of an abscess in the breast. I concluded she was suckling a child, but I was mistaken. Anxious to ascertain the cause of so unusual an occurrence in an apparently healthy person, I made every inquiry into her past recent history. Have you had a blow on the breast? No. An attack of illness? diarrhœa? any unusual or excessive loss of blood? any great muscular effort or loss of food? All these questions she answered in the negative, and I was about to relinquish my inquiry when the lady said "Perhaps I ought to tell you that a few weeks ago I sat up seven successive nights with a dying child." I thanked her for such important information, and the mystery was solved. Why should this lady have abscess in the breast? She had not borne a child for many years. There must be some cause for abscess, local or general. It might have been produced by a blow, but it was not. Investigation proved it to be the result of sudden reduction of her vital powers by great physical and mental effort. Why it assumed this particular form I know not. I only know that a great cause of derangement of her system had occurred, and the consequence fell on that particular organ. The late Lord ——, whose recent and sudden death caused so painful a sensation, became the subject of phlebitis consequent on severe bodily exercise with a pulse of 50, which no amount of stimulus could arise above it so long as he subjected himself to this daily effort. Here is exhaustion as the cause, phlebitis the effect, and sudden death by embolism.

Inflammation of the uterus or of the venous system below it follows hemorrhage in parturition. Accoucheurs with large experience will confirm this fact.

Many years ago I was present at the attempt to reduce a dislocation of the femur in a London Hospital. The man who was to all appearance in perfect health, was bled largely from both arms and from the temporal artery. He also took tartar emetic to the extent of some fifteen grains. The quantity of blood abstracted from his circulation must have rendered the functions of the heart a sinecure, for it amounted to 120 ounces. He survived a week, and died of phlebitis, as I had predicted.

I might multiply these examples, but it is unnecessary if you comprehend the force of the cases I have quoted. The causes of disease I refer to above may be classed as follows:

1. Loss of blood, whether accidental or effected at the hands of science; catamenial, hemorrhoidal, &c.
2. Loss of food, or failure of the material of blood making.
3. Excessive purgation, whether natural and spontaneous, as in diarrhœa, or artificial at the hands of science, carrying off nourishment.
4. Breathing impure air, by which the quality of the blood deteriorated.
5. Loss of sleep.

6. Excessive muscular effort, as in walking great distances, boat-racing, etc.
7. Extremes of temperature, especially of cold.
8. Great mental emotion or mental shock.
9. Protracted anxiety of mind.

Of the above sources of subsequent illness or disease, the influence of the four first is received more directly by the vascular system, the five latter either partially or entirely by the nervous.

Before speaking of the general treatment of disease, to which I shall shortly come, I wish to say a few more words on the subject of some prevailing doctrines which are so generally adopted by our Profession, and the entire soundness of which appears to me yet open to question and inquiry. Why do you order an aperient in nearly every case of disease you are called to see? If the patient is strong and vigorous, the dose is a full one—if weakly, a milder form; but always an aperient, and generally it is combined with mercury, which indeed forms the staple of the medicine all but universally prescribed. As we commonly go at once to the cause, and prescribe our best remedy, the natural inference is that there is either liver or intestinal derangement as the cause of the disease. You say you desire to unload the liver and to remove extraneous and irritating matters from the intestinal canal; and very good treatment too if the liver is at fault and the alimentary canal demands that kind of relief. But what evidence have you of it? You say the liver is congested. It is a term in the mouth of nine-tenths of the profession practising medicine and surgery in the dominions of her Majesty the Queen. The practitioners on the Continent of Europe take a different view of these matters, and not without some show of reason, for English doctors, though I entertain the highest respect for their attainments, do not monopolise all the knowledge of the world. In discussing the subject, I am prepared to acknowledge the occasional presence of constipation as the result of torpid action of the bowels, in which condition the liver may, or may not, be involved; but I am myself unable to detect what appears so obvious to others—a congested state of this organ calling for large doses of chloride of mercury, supposing that form of drug to be the best corrective of the evil. As I believe a person who is the subject of disease, itself so commonly the product of exhaustion, should not undergo further reduction of his strength without a good and sufficient reason, and as I doubt the congested state of the liver and find the torpid condition of the large intestine, if it exist at all, an evil on a small scale, I prefer to look to the disease itself (suppose it, if you please, erysipelas or any other malady), and if the pulse is soft and compressible indicating distinct constitutional weakness, *and not otherwise*, I prescribe at once a tonic remedy. I look upon this torpid condition of the large intestine—for, observe, nearly all constipation is limited to this part of the alimentary canal—as merely a symptom of the general debility, and increased by loss of appetite and the absence of food, and is not to be rudely and violently assaulted by drastic purgatives; and one of the first signs indicating the sound principle of a tonic treatment will appear in the gradual, but certain, restoration of the functions of the alimentary canal to a state of health. Constipation is very commonly, though not invariably, the concomitant of weak health and low vital power; and is caused by defective power of the muscular fibres of the large intestine, which are, when compared to the small intestine, very limited in quantity in relation to the size of the intestine. You treat constipation by means of purgatives which act on the mucous membrane only. I prescribe iron to give tone to the muscular coat. Your treatment affords a temporary and transient benefit; mine is a permanent one. Which will you prefer? And this subject leads me to add a few words on the treatment of disease adopted by my own respected teacher, Mr. Abernethy, who upheld the doctrine that health demanded the daily action of the bowels, and that the liver was an offending organ and demanded the daily administration of a stimulant or a supposed provocative to the secretion of an increased quantity of bile. No doubt Mr. Abernethy's treatment was to an extent successful because, as the tendency of the large majority of diseases is towards recovery at the hands of Nature, his cases prospered like those treated on other principles. In these days one does not quite comprehend how a man of Mr. Abernethy's grasp of intellect could delude himself with the idea either that disease could so generally depend an torpidity of the liver, or on imperfect action of the muscular coat of the large intestine. If it be necessary to obtain as the requisite condition of health a daily action of the bowels, why did nature give us large intestine sufficient to contain at least a week's consumption of food.

In cases of extreme weakness good service is often rendered by "locking up" the bowels, and reducing their action to that of alternate days.

I consider the treatment of the great majority of diseases to consist in increasing the quantity of healthy blood and giving force to the action of the heart. *You can't cure disease with a feeble pulse.* Mend the pulse, and Nature will do the rest of the work. On this principle disease in general may be treated, so far as my observation has gone, with pre-eminent success. In order to appreciate fully its force, you must start with the conviction that Nature cures and not man—man removes obstructions from her path, and nothing more. This done, he awaits the onward move of the great machine, like to a great ship of gigantic weight, which, quietly held in her position at rest by a few timbers, immediately obeys the great natural law of gravitation on their removal, and glides into the water below. Did man launch this vessel, or did Nature? With as much title may the Physician or Surgeon declare that he cured a disease. There are of course occasional exceptions to this assertion in some cases of operative surgery. The object of treatment is to restore the pulse to its normal standard of force and frequency. Give it due force, and the heart will determine the number. As a rule, in cases of debility, it is too frequent, and frequent because the quantity of blood in the system is below the standard of health. Increase the quantity, and the pulse falls. Assure yourselves of this. Unthinking persons jump to the conclusion that brandy or other stimulants necessarily raise the pulse, but this supposes that we start with a healthy pulse at par. I am talking not of health but of disease. In my capacity of Examiner at the College of Surgeons, I often put this question. If you take a pint of blood from a healthy man of 40, with a standard pulse of 68, what effect will be produced on the number of pulsations by the loss? What do you imagine is the frequent reply? "It reduces the number to 60!" And this curious answer explains something of the phenomena of venesection so universally practised some years ago, when in reporting on a case it is said "His pulse rose on bleeding, so I bled him again." As a rule, you will find that whenever the frequency of the pulse is above the standard of health, *as an indication of debility*, a stimulant will reduce it. I tried this experiment, or rather I obtained this test, for it was not an experiment. On coming out of a Turkish bath of something more than the usual intensity of heat, my pulse had risen to 90; I drank about two ounces of wine, and my pulse fell to 75 within a few minutes.

I have something more to say on common matters relating to the routine of every day's practice. There is great repugnance prevailing in the medical mind to the employment of stimulants. It is, perhaps, mainly founded on the moral evils of excess. Inebriety is a low vulgar vice, and therefore the agents producing it are morally objectionable and medically injurious; but you must draw a distinct line between health and disease. In health I am the advocate, both in precept and example, of moderation, in disease not. I think one reason why stimulants are so little resorted to in cases of debility is because, being administered in hesitating quantities of doubtful utility, their value is not appreciated. You must not guage the capacity for alcoholic stimulants in disease by the capacity of the same person in health. Those only who adopt this treatment as a principle are cognisant of the remarkable tolerance of stimulants under great prostration of the vital powers, and when required as the antidote to prostration I maintain they are perfectly harmless. It is the remarkable tolerance of alcoholic stimulants in these persons to which I wish to direct your earnest attention. You can't intoxicate these people; you cannot even unduly excite them. It is a common remark among observant persons under treatment, "I drank three glasses of wine, but it had no more effect on me than so much water;" whereas less than the same quantity during health would have produced partial intoxication, or at all events excitement. These are the persons who demand the free and fearless resort to stimulants, and to whom alcohol is life. Do not, I again urge upon you, measure the quantity to be administered by the glass. Gauge it by its effects, and so long as weakness prevails, indicated by the pulse, persist in its use; and as you proceed in your treatment, and the services of the wine become more and more palpable, you will not fail to see that its consumption becomes less and less essential to onward progress, and in the course, it may be of months, of weeks, or even of days, you will reduce the quantity to the standard of health, for the capacity for wine has passed away with the disease that claimed it. You need never listen to the objection often urged that the resort to stimulants will become habitual; be assured it is entirely groundless. Now listen to the following examples of this principle of treatment. A young lady whose daily consumption of wine has never exceeded two glasses, oppressed by the heat of a crowded room, faints. The heart and the brain have failed in their functions by reason of the imperfect oxygenation of the blood. Recovering from the first stage of sudden depression of her vital powers, her symptoms pass into those of hysteria. We have all witnessed cases of the kind. Ordinary and mild stimulants are unavailing, and make no impression. An hour, two hours, have elapsed, and she is yet prostrate. Before her recovery this girl has consumed three wineglasses, or upwards of half a pint, of brandy and other cordials, and often more. What is the explanation? What is the physiology of this? Had you administered this quantity to your patient during health, you would have done her a serious injury. She now takes it with benefit and with impunity. The shock to her system was for the time dangerous; the stimulant, given at another time, would have proved dangerous also; the two united are harmless. The one has balanced or antagonised the influence of the other. There remains neither headache, nor nausea, nor any evidence of excess, because all the stimulating power of the brandy was required and consumed in re-establishing the circulation. Again:

—A gentleman of abstemious habits, ill clothed for the occasion, ventured, in a cold season of the year, to mount to the summit of a mountain in Wales. He was seized with intense and sudden prostration from the effects of the temperature. His suffering was severe. At this opportune moment, a friend supplied him with a half-pint bottle of raw brandy, every drop of which he drank at a draught, as he would have drunk a glass of table beer. The only effect produced on his system was warmth. He drank more brandy at that moment than he had ever before consumed in a week. No evil resulted. The cold and the brandy being in antagonism, each entirely neutralised the injurious effects of the other. I need not say that a similar draught, unprotected by the depressing influence of the intense cold, would have proved almost dangerous to life itself—certainly most injurious to health. I attended in his last illness the late Duke of ———, a man of remarkably abstemious habits. His malady was a mortal one. Some months prior to his death he became the subject of frequent attacks of rigors, which continued with great severity for two or three hours, and left him in a condition of extreme prostration. I called one day opportunely, and found him labouring under one of these attacks. I gave him two-thirds of a wineglassful of raw brandy without effect. In five minutes I repeated it, and he took a wineglassful of brandy every seven or eight minutes till the attack passed off. He had then consumed three-quarters of a bottle of pure brandy within an hour. I saw him in four hours afterwards, apparently well. In reply to my question, "How do you feel?" he answered, "Well, I think you gave me pretty large doses of brandy, but I cannot say I feel in any respect the worse of it." Nor was he. Throughout his subsequent illness he had no attack of rigors. The prostration of the vital powers in this case must have been immense. One of the last cases attended by a late eminent physician—now, unhappily for the cause of medical science, lost to the profession—was a gentleman of about 30, who was the subject of hydrothorax. His case was urgent, and I was requested to see him. Concurring in opinion with the physician in attendance in the necessity of giving immediate relief by operation, I punctured the chest and got away six pints of serous fluid. The cavity thus made was entirely occupied by atmospheric air, to the admission of which I have never yet ascertained the grounds of objection or seen any injurious result therefrom. I have done this operation often, and have never considered it desirable to exclude the air from the cavity within. Though irrelevant to my subject, I cannot forbear asking the simple question, What is supposed to occupy the space from which the fluid has been removed? Certainly the lung will not immediately expand, and the walls will not collapse for a period of weeks or months. At the conclusion of the operation the patient was much exhausted, with a pulse of 130. It was a great object in the treatment to reduce it. What means would you have adopted? what does professional usage demand? Salines, think you, or purgatives? No, on the contrary. He took the only remedy that could have probably saved his life. He was ordered 1 oz. of brandy diluted with an equal quantity of water every three hours, and on the day following his pulse had fallen to 90, and he recovered. If this treatment was not sound, it ought to have proved fatal. I have abundant examples at hand of the efficacy of this principle, but I have quoted sufficient for my present purpose. They go to exemplify this therapeutical principle—viz. 1. That stimulants alone can restore the vital powers under great and sudden prostration; and 2. That under great and sudden prostration the capacity of the system for stimulants is enormous, and may be administered to almost any amount with safety.

Now, gentlemen, in the prosecution of your profession, while engaged in the study of by far the most critical department of medical education—the nature of disease, or diagnosis—have you ever thought of the important distinction which exists between the diseases of animal and organic life? And yet they are worth considering. A few years ago I made some calculations on the subject of the relative quantity of blood supplied to each. I have not these calculations at hand, but the quantity of blood so distributed is nearly equal, though rather the larger proportion supplies the structures of animal life.

The nerves, as you know, are essentially different, animal life being spinal, organic life ganglionic. If we take the whole material of the body into the calculation, the structures of organic life do not exceed one-tenth of the actual weight, yet they receive nearly one-half of the whole quantity of arterial blood.

The "two lives," as they were termed by the great French physiologist, Bichat, are more or less independent of each other. both in health and in disease. The diseases of organic life are more serious, because they involve structures more essential to life itself, and the derangement of any one of them involves the whole system more thoroughly than does a similar amount of disease in the structures of animal life.

You will hear a man say "he can't have much wrong with him," because he is capable of great exercise, and can walk any reasonable distance. This is a bad test. So long as the muscular system is not directly involved, and sufficient blood is made to supply both it and the spinal cord from which it derives its nerves, a man may labour under a considerable amount of disease of the structures of organic life without his muscular system being involved. When we speak of a man's "health" in general terms, we refer instinctively to the functions of the interior of the body or of the organs, not of the muscular system; we refer to the functions of the brain, to respiration, to circulation, to digestion, and the assimilation of food, &c. The muscular is but subservient to these other organs which really constitute life itself.—*Medical Times and Gazette, July* 21, 1866, *p.* 55.

7.—ON CATCHING COLD.

By Dr. THOMAS INMAN, Physician to and Lecturer on Medicine at the Liverpool Royal Infirmary.

Catching cold is a common phrase for an attack of catarrh, but it is a very incorrect one.

One year I suffered so very severely from a series of "colds" that my attention was drawn specially to them. I was then Lecturer on Medicine, and nearly every night from five o'clock to six during the winter months had to turn out from a warm room to go through all weathers, lecture for an hour in a theatre heated by a stove and lighted by gas, and then return again to my snuggery at home. When I felt a fresh cold beginning, I tried in vain to account for it, until I accidentally saw in Copland's dictionary that the most fertile cause of a cold was coming from a moist cold air to a hot and dry room. This at once explained to me the reason of my frequent suffering, for I had invariably gone into my hot room straight from the cold. I, of course, soon changed my habit; I dawdled in the hall while taking off my great coat, perambulated the rooms which had no fire in, went up and down stairs and the like, ere I went into my study, whose temperature was also reduced. Since then I agree with a friend who says, "that a cold comes from catching hot;" and I am disposed to think that there is a strong analogy between a chilblain on a child's toes and a cold in a person's nose, throat, and lungs.—*Medical Mirror, Aug.* 1866, *p* 460.

8.—ON THE TREATMENT OF RHEUMATIC FEVER.

By Dr. J. BIRKBECK NEVINS, Liverpool.

[The fundamental principle in the treatment of rheumatic fever is that recommended by Dr. Heberden above a hundred years since, viz., the employment of cinchona, in consequence of the resemblance between rheumatic fever and ague, as shown in the rigors, hot and sweating stags, and tendency to periodicity in both diseases.]

Case.—W. J., aged 45, a delicate looking man, steward of a steam-ship, had been suffering from rheumatic pains for a fortnight, but had gone about his work with difficulty until two days before his arrival in Liverpool, during which he was confined to his berth, unable to help himself in any way. He was carried on shore, and I saw him in the evening. He was unable to turn in bed, or to move hand or foot, except his left hand a little, though even that was acutely painful. He had had no sleep for two days or nights ; tongue furred ; lithates in urine ; pulse moderately excited, but no distinct heart-symptoms ; not much sweat.

Treatment.—He was immediately ordered a vapour-bath of vinegar, with subsequent cold douche in bed, and ten grains of Dover's powder, which was followed by two hours of sleep, and such abatement of pain, that he said he was easier the next day, though still unable to move his limbs or to turn in bed. He was also at once ordered two grains of quinine and five grains of iodide of potassium, to be taken four times a day. He had, on a subsequent night, a second Dover's powder, and this was all the opiate taken during the illness. The opium, therefore, formed a very insignificant part of the treatment ; and this I have found to be the case almost without exception.

The remedies to which I attach importance are :—

1. The vapour-bath, and subsequent cold douche ; and,
2. The combined quinine and iodine.

In this case the bath was given in bed, for the patient could neither turn in bed nor move his limbs ; and it will generally be necessary to give it in bed, in the first instance, in any case deserving the name of rheumatic fever ; and it is so easily administered, that no difficulty can arise to prevent its employment in every case.

Two large pieces of coarse flannel (common scouring cloths answer the purpose admirably) are to be soaked in common vinegar ; about a pint being necessary for each cloth. Two common bricks are then to be heated nearly red-hot in the fire, folded up in these flannels, and placed on two plates. The patient being stripped, one plate is to be put a little distance from one knee and the other a little distance from the opposite shoulder, and the patient is to be covered over with the bedclothes. In a few minutes he is surrounded by a most refreshing steam-bath, which produces a warm, agreeable perspiration, that may be kept up for twenty minutes or longer, if the bricks retain their heat sufficiently.

As soon as it is decided to remove them, the patient, still in bed, is to be very rapidly mopped all over with towels wrung out of cold water, then immediately wiped dry with dry towels, supplied with a warm shirt or flannel garment, and covered with a fresh dry sheet, &c., or with blankets alone, as may be most agreeable to him.

The effects of this bath are a speedy relief of the acute pain, and frequently easy sleep for a time; an abatement of the offensive and distressing acid sweats; and a general state of greater comfort.

The cold water application immediately on the removal of the hot vapour is very important; as it prevents the continuance of an enfeebling perspiration after the hot bath.

The manner of removing the patient's bed garment is a point of importance in cases of such painful helplessness as rheumatic fever; and it is accomplished without pain to the patient or difficulty to the nurse by an extremely simple contrivance. The clothes must be torn down the back from top to bottom; and when this is done they can be removed and replaced as easily as a child's pinafore, without even lifting a limb of the patient or disturbing him in bed. By this means, fresh, clean, dry clothing can be applied without difficulty once or twice a day, according to the amount of sweating; and the sufferer is relieved from the discomfort of his damp, offensive garments.

This bath may be repeated twice a week; and during seventeen years that I have been in the habit of adopting it, I have scarcely ever had to use it a third time in bed; the patient, after the second bath, being almost invariably able to sit up and have the third in a chair.

When he is able to sit up, a steam-bath can be given with great ease by putting a bucket of boiling water under a chair, the seat of which is sufficiently protected to prevent the patient from being scalded, whilst he is sitting upon it surrounded by blankets; and, by putting a red-hot brick into the water in the course of ten minutes, the steam is kept up, as by this time it generally begins to abate from the original boiling water.

A jug of cold water may be poured over the patient when the blankets are removed, or he may be wiped by cold wet towels, as is most agreeable to his own fears or feelings, and he must then be clothed and sit up for a few hours.

The second part of the treatment upon which stress is laid, is the combination of moderate—*i.e.*, two grain doses of quinine with five grain doses of iodide of potassium from the first. The theoretical grounds on which quinine was first proposed have been already mentioned; and the general experience of the profession will suggest the explanation of the probable benefit to be looked for from the addition of the iodine.

We will now return to the history of the case.

After using the bath and taking the Dover's powder, he slept two hours, and was easier.

Second Day of Treatment.—Tongue rather dry. Two glasses of wine daily, in addition to his medicine.

Fourth Day.—Sleeps moderately, and takes food moderately.

Very uneasy from lying so long unable to turn in bed. Can move one arm a little. Repeat the vapour-bath, and continue quinine and iodide.

Next Day—Fifth.—Can sit up in bed, and move his arms so as to change his night-shirt in the ordinary way.

Seventh Day.—Walked down stairs with a little help.

Tenth Day.—Had a steam-bath in his chair.

Eleventh Day.—Walked a mile and a quarter.

Twelfth Day.—Went down to the office.

Sixteenth Day.—Called upon me just before going to sea.

Such is an outline of the plan of treatment which I have practised habitually for the last seventeen years. During this time the cases have been numerous which have been thus treated; and the results have been so satisfactory, that I have always returned to this method, although I have given a fair trial to the alkaline and to the lemon-juice treatment. I have not tried the do-nothing method; nor have I ever relied upon opium alone; and bleeding and mercurials I have no experience of.

During this period I have only had occasion four times to apply a blister for heart-symptoms; and there has not been any instance of troublesome cardiac affection. What has become manifest on these four occasions has readily yielded to slight blistering, and a continuance of the quinine and iodine.

When the disease previous to admission has been of a more chronic or frequently repeated character than in the case above related, the improvement has not been so rapid as to amount to complete recovery in a fortnight; and where there is much gouty complication, the case will probably be more lingering. But, after endeavouring to ascertain without partiality what method of treatment is most beneficial to the patient suffering from rheumatic fever, I am increasingly impressed with the conviction that the plan now advocated possesses the advantage of—

1. Relieving the patient's suffering most speedily, both as regards pain, loss of rest, and sweating;

2. Of most quickly restoring the patient to strength, for it is extremely rare for him to be confined to bed more than a week, or to be confined to his room for more than a fortnight; and

3. Of securing extraordinary freedom from heart-complications, or liability to relapses.—*British Med. Journal, Sept.* 8, 1866, *p.* 273.

9.—HOW LONG DOES RHEUMATISM TAKE TO GET WELL OF ITSELF?

By Prof. Bennett, Edinburgh.

Who could venture to say that this or that treatment was or was not successful, if he did not know how long the disease was likely to last of itself? He had tried everything—there was no drug he had not tried—in rheumatism, and he found them all pretty much alike. He had tried nothing, and the result had been the same. He had tried the blanket, with the same result. They had given the alkaline treatment a most careful trial in Edinburgh, and it had seemed to him to have shortened the duration of the disease a little. In Edinburgh they saw a great deal of rheumatism in servant girls, on account of the system of washing adopted there. Girls got up very early in the morning to wash, and in all weathers, without care as to clothing, they alternated carelessly between the hot steaming kitchen in which the washing was done and the open air, often bitterly cold, where they hung the clothes to dry. He thought he had observed that where a strong young girl of this sort was seized with rheumatism, the acute symptoms had a tendency to disappear in about six or seven days; and if there was much complication, it would disappear in about 14 or 15 days. If the girl were weak, they found that murmur at the heart for which formerly violent remedies—mercury and so on—were given. Then, as, under the influence of rest and good diet, and warm comfortable treatment, the patient got stronger, the murmur went away. In the old time, when mercury was given in these cases, it generally so happened that the murmur went away about the same time as the mercury touched the gums; and so it was concluded that the murmur went away in consequence of the mercury touching the gums. Now mercury was not given, and yet the murmur went away as soon. They had come to the conclusion in Edinburgh that, in one of the healthy cases he had named, if alkalies were used—they gave nitrate of potash—instead of being seven days, the average duration of the acute symptoms was six days and a half. He had given nitrate of potash regularly ever since coming to that conclusion. His view was that acute rheumatism cured itself. Dr. Warren, of London, once said that six weeks in bed was the best cure for everything; and he had noticed that of Dr. Stewart's cases on the previous day, the average duration was forty-two days, which was very near six weeks. —*Medical Times and Gazette, Sept.* 1, 1866, *p.* 232.

10.—ON A NEW METHOD BY WHICH MALIGNANT TUMOURS MAY BE REMOVED WITH LITTLE PAIN OR CONSTITUTIONAL DISTURBANCE.

By Dr. W. H. Broadbent, London.

The attention of the author was directed to the treatment of cancer under the following circumstances:—In 1864 he was consulted by a lady suffering from cancer of the breast. By his advice the breast was removed by Mr. Walter Coulson. The disease returned, and was again removed in August, 1865. In May of the present year, a tumour was growing more rapidly than ever near the cicatrices of the former operations. It was decided that no further removal was advisable: and, unless something could be done, a miserable fate was before the patient. The hypodermic syringe is now in the hands of every physician; and it seemed to the author that by it some fluid might be injected into the tumour which might so far alter its structure and modify its nutrition that its growth might be retarded or arrested. After considering the various substances which presented themselves to his notice, he selected acetic acid, for the following reasons:—1. This acid does not coagulate albumen, and might, therefore be expected to diffuse itself through the tumour; and the effects would not be localised at the point injected. 2. If it entered the circulation it could do no harm in any way. 3. Acetic acid rapidly dissolves the walls and modifies the nuclei of cells on the microscopic slide, and might be expected to do this when the cells were *in situ*. 4. It had been applied with advantage to common ulcerations. On May 18 the first injection was practised. The tumour was of about the size of a small egg, and a patch of skin of about the size of a shilling had become adherent to it. The needle was introduced through sound skin an inch or more from the part involved in the disease, and passed to the centre of the mass. About thirty minims of dilute acid (one part of acid to one and a half or two of water) were injected. It gave little or no pain. Next morning a bulla containing dark bloody fluid was found to occupy the patch of adherent skin. May 23: This portion of skin dry, hard, and horny; the adjacent part of the tumour not so hard. Again injected. The patient, residing in the country, was not again seen till June 7, when the piece of skin mentioned was found detached from the surrounding sound skin; and a probe could be passed in all directions to a distance of three-quarters of an inch or more between the tumour and the healthy structures. A little discharge issued from the fissure mentioned. Injected on this date, and again on the ninth, the acid used being rather stronger. It gave a little pain, and swelling and tension of the parts around followed. On June 13, and a few days afterwards, there was a free discharge of fluid and solid portions, with relief of the swelling, &c. No fœtor whatever attended this discharge, which afterwards diminished greatly. Seen again on June 26, when, on external examination, the tumour was found to be much smaller; and, on passing a probe into the opening, it entered a large cavity extending on all sides. Part of the walls seemed free from malignant structure, but at several points a crust of cancerous deposit remained. On attempting to inject, it was found too thin to retain the fluid, which either entered the tissues and gave great pain, or made its way into the cavity. The cavity was stuffed with lint saturated with dilute acid, and the case left in the care of the family medical attendant, who was to inject as he saw opportunity. July 13 : No impression made on the remaining disease, which had, in the opinion of the medical man, extended somewhat. Carbolic acid was tried for a few days as an application, but discontinued, and the cavity dressed daily with strong acetic acid by the medical attendant, and injections practised daily. This energetic treatment gave much pain, and excited inflammation all around. When again seen by the author on August 4, there had been considerable hemorrhage, which had been arrested by free application of tincture of sesquichloride of iron. The result, however, was apparently the entire removal of the remains of malignant disease ; and, when last seen, a healthy granulating surface was left at every point. Three other cases were related by the author. The author further formulated certain conclusions from the experiments detailed, and stated the cases to which, in his opinion, the treatment was not applicable. Guided by his experience, he considered large quantities of dilute acid preferable to stronger acid ; and he would not, without great hesitation, attempt the destruction of any tumour which had not involved the skin. His aim had originally been, as stated in the early part of the paper, not necrosis of malignant tumours, but a modification in their nutrition. The theoretical grounds for this hope were, that cancer owed its malignancy to its cellular (to use a nomenclature now almost antiquated) or fœtal structure ; and that in acetic acid we had an agent which might be expected to diffuse itself through the tumour and reach the cells, and, having reached them, to effect changes in their structure, and affect them vitally, while it could scarcely do harm. The results he had brought before the profession at the earliest possible moment. The ultimate value of the treatment he left to be decided by a more extended experience. It was important tó use large quantities of dilute acid, and not to have the acid too strong.—*Medical Times and Gazette, Sept.* 1, 1866, *p.* 229.

DISEASES OF THE NERVOUS SYSTEM.

11.—ON SOME FUNCTIONAL DISEASES OF THE NERVOUS SYSTEM.

By Dr. Samuel Wilks, Physician to Guy's Hospital.

[Dr. Wilks considers that a more scientific classification of nervous diseases may be formed by connecting their phenomena with morbid changes in the nervous centres. He does not, however, suppose that we can expect to find a tangible change, discoverable in the post-mortem room, in every case in which nervous symptoms have been present during life. The present paper treats only of some functional disorders in which of course no organic change is discoverable. The nerve centres may be exalted or diminished in function, and these altered conditions may either be temporary and speedily recovered from, or followed by death in a direct or indirect manner.]

In women at the climacteric period we see the effect of lowering of the nervous influence in the fluttering of the heart, and the sickness, sighings, headache, &c. Probably no other invalids really feel so ill as these patients; the whole bodily functions are disturbed, and consequently a depression is experienced far exceeding that which accompanies any real organic disease. Such patients describe their feelings with the utmost despondency; they experience successive changes of temperature, which they style flushes of heat; they complain of anorexia, of flatulence, and of irritation of the bowels, uterus, or urinary organs. In fact, there is not a single viscus which does not suffer disturbance, so that every disease in the nosology may, in turn, be supposed to be present. Why one organ should suffer more than another, or why a morbid sensation should be experienced in this part of the body rather than in that, is probably to be explained by the anatomical distribution of the nerves; but we certainly find that, probably owing to the large supply of the sympathetic nerve in the abdomen, greater depression is experienced in abdominal than in other forms of disease, and that in all low conditions of the nervous system morbid sensations are very often referred to this region of the body. Thus, we cannot but contrast the cheerful disposition of the phthisical patient, when on the brink of the grave, with the depression observed in one who has but a temporary disturbance of his stomach, liver, or bowels; and another indication of the same fact is the placing of the emotions in these parts, as expressed by the term "bowels of compassion."

Under the most varied conditions, both in men and women, when life is low, a number of morbid sensations arise. In a state of health man should be as happy and joyous as the lark flying in the heavens; he should have a keen sense of animal enjoyment, and he should feel nothing of the working of the machinery within him; but when his nervous system is depraved he becomes conscious of all these movements, he feels his heart beat, his head throb, and his back ache. A study of these nervous symptoms would probably show in what order they appear; I think that they commence, especially in women, with a pain in the left side, and that this is followed by pains on the top of the head, and in the back, at the epigastrium, over the collar-bones, &c. I feel uncertain whether these pains are altogether subjective or due to some prior alteration of function in the parts whence the pain proceeds. This question, however, is one of very great importance in practice; for we often find that, by attempting to relieve symptoms, we gain no headway towards overcoming disease, whilst by altogether disregarding them, and having recourse to a general tonic plan of treatment, we can ensure a cure within a certain period. At the same time it cannot be denied that the application of remedies to the spot to which the morbid sensation is attributed is frequently attended with success. Thus, plasters to the side, sedatives to the stomach, &c., do give relief. Relieving the local symptoms in this way is not, however, incompatible with a treatment directed to the restoration of the nerve centres themselves.

In practice, we have almost every hour of the day to endeavour to discover whether morbid sensations in and disturbances of the viscera are due to an organic cause or to the mere failure of the regulating power of the nerves; that is, whether the disease is organic or functional. In such cases the diagnosis is doubly difficult in females, because symptoms resembling those of almost every form of disease may be produced by their more delicate nervous organization.

We can imagine, by way of analogy, that in the case of a steam-engine working irregularly we might for a time be at a loss to discover whether the derangement was owing to some material deficiency in the valves or joints, or whether it was due simply to an irregular supply of steam. Or, again, we can picture to ourselves a clock perfect in all its parts moving too slowly, from the simple fact of the weight having nearly run down.

In the cerebro-spinal system, again, an exaltation or a depression of function is constantly witnessed. Such conditions are observed in chorea, in hysteria, and in various passions of the mind. Müller speaks of the nervous principle in the medulla oblongata as being in a state of tension and always ready to act, and he says that the slightest change in its condition excites a discharge of nervous influence, as is manifested in laughing, sneezing, &c. Thus every mental impulse to motion disturbs the balance of this tension and causes a discharge of nervous influence in a determinate direction. He also compares the nervous system to a musical organ, with its bellows charged and ready to force a stream of air in any direction, according to the particular key that may be touched. Using this illustration, we may imagine the air either to rush out with a scream, or to be hastily allowed to pass off by the larger tubes, or to be diffused melodiously through a series of musical pipes. In a similar way the superfluous nerve-force may display its operations in various ways, according to the sex, age, and temperament of the patient. For example I have seen the same cause produce hysterics in a mother and chorea in her child, the one disease being almost peculiar to the adult period of life, the other to childhood. The same fright which excited so great an amount of nerve-force in the mother as to cause the explosion known as hysterics operated on the child in a slower manner, and gave rise to the less violent action known as chorea. The spinal system was excited to over-action by the cineritious substance above, which had been unduly stimulated by a mental shock, and remained temporarily impaired until the disease was cured. The explosion of nerve-force by an hysterical attack acts as a kind of safety-valve. protecting the internal machinery from danger; and although all are not alike impressionable, there is scarcely an individual who may not be in need of it when acted on by a sufficiently powerful stimulus. Even in the strong-minded Napoleon a fit is said to have been excited by passion. More commonly, however, relief to an over-excited nervous system is afforded by laughing or crying. Thus as Byron observes, the power which women possess, as compared with men, of being able to pour their troubles into their pocket-handkerchiefs, is no doubt often very beneficial to them, so far as their health is concerned. A woman who is excited, if she do not go into hysterics or have a good cry, often allows the redundant nerve-force to escape through that unruly member the tongue, and thus an extreme volubility of utterance perhaps saves her from further unpleasantness. Of course, the talk which flows from her lips is altogether different from the result of an intellectual process; and thus it is still true now as it was in ancient times, that "anger is a short madness." In Switzerland, last summer, I met an Irish gentleman, who told me that he could make his wants known without a knowledge of the language, but that when he was irritated and wanted to swear he would sometimes give all he possessed to understand German. In other cases, again, the superfluous force escapes by the limbs; thus, an angry person slams the door, or destroys even her own property. A man of better sense, when vexed, takes a walk, and thus gets rid of his extra nerve-force; or, if the irritation and its results are more chronic, sits down, takes up his pen, and by publishing "the whole correspondence" eases his mind. —*Guy's Hospital Reports, Vol. XII.*, 1866, *p.* 247.

12.—ON HYSTERIA, OR GENERAL AND LOCAL NERVOUS IRRITATION.

By F. C. Skey, Esq., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

In the whole range of practical Surgery there is, perhaps, no one subject that claims your earnest study more important than that which I have selected for this and the following lectures. It is not a question of diagnosis between two diseases more or less resembling each other. It is a question of disease or no disease, of reality or imitation, of true or false—of whether your purgatives, your bleedings, sweatings, irritants and counter-irritants, and your whole battery of antiphlogistics, shall be launched against a true disease in the flesh, or its ghost—whether you are to contend with a reality or a shadow. This absence of discrimination between two conditions of disease and no disease are painfully frequent among medical men, especially among those to whose charge is assigned the care of local and surgical diseases. "In one shape or another," observes the greatest of modern surgeons, "you will meet with them at every turn of your future practice." It may be asserted with truth that every part of the human body supplied with nerves, be they cerebral, spinal, or ganglionic, may become under provocation the seat of local symptoms so closely resembling those of the real disease to which that part of the body is liable, as to appear identical with it, and the resemblance to which is so perfect as to deceive the best of us. They are not cases of occasional or rare occurrence. They come before us in the daily and hourly walks of professional life. They monopolise a share, and not a small one, of all cases under treatment, whether medical or surgical, but the latter predominate. The closer you scrutinise them, the more penetrating your inquiry—looking into, and not at them—the more perfect will be your diagnosis, and the more will you be astonished that a form of disease so remarkable and so common should have hitherto occupied so little of your thoughts. It is well to call your attention to this description of malady at the early stage of your career. Many men pass through life, engaged in active warfare against disease, on whose convictions this variety has scarcely dawned. And this is a truly remarkable fact, which owes its existence to the predominating influence which the heart and the arterial system exercise over the judgment of the profession at the expense of a system yet higher in the scale of organization, more sensitive, and more liable to morbid impressions—viz., the cerebro-spinal nervous system.

Whenever a new case of disease presents itself to us, we jump to the old doctrines of inflammation, we talk of congestion and of capillary action and of deposits of lymph, and we refer the attendant pain and heat to an inflammatory condition of which the local nervous derangement is an ordinary symptom. We should endeavour to assign to each system its proper place in the pathological scale, and to discriminate more accurately than is generally done the indications which belong to the morbid conditions of each, whether existing in combination or separately. For be assured they do exist, both separately and in combination with each other. You may have varieties of inflammation in which the local pain is trivial when compared with its severity in other cases; while, on the other hand, examples daily occur in which local as well as general derangement of the nerves, whether of the part or of the whole body, exists as a condition entirely independent of the vascular system. Nor is this derangement confined to the sensory nerves. If we have local pain as the indication of excessive activity of the nerves of sensation, we have spasm and convulsions indicating derangement of the nerves of motion, each of which, or both, may prevail without heat, or redness, or swelling. We daily see severe forms of nervous exacerbation without the slightest corresponding increase of action in the vascular system. There is this important difference between the morbid states of the vascular and nervous systems, that while local inflammations are dependent on local causes, aggravated only by the impaired condition of the general health, local nervous diseases for the most part originate in the centres of nervous power, the effects of which are exhibited in remote parts of the body—it may be in a pain localised in a given spot, whether on the surface or in deeply-seated parts, which to our senses holds no especial relation to its nervous centre; it may be in a temporary, or spasmodic, or permanent contraction of the voluntary muscles bending the joints of the extremities in permanent flexion, or obliquely drawing the head upon the trunk, or involving the whole motor system, as in tetanus. No known nerve that conveys sensibility from its centre to its periphery—no motor nerve that carries volition from the brain or spinal cord to a voluntary muscle—is exempt from this morbid tendency.

The vascular system, consisting of arteries, capillaries, and veins, has its own special diseases peculiar to the structures engaged in the circulation of the blood. The attendant symptoms are heat, redness, pain, and swelling, the latter symptom being due to a separation from the capillary system of some constituents of the blood, whether in a fluid or solid form, while the morbid conditions of the nerves and the structures in which they originate are characterised by simple aggravation or excess of the functions of the nerves affected, the natural sensibility of the sensory nerves running into pain, and the moving power of motor nerves into convulsions, or spasm, or permanent contraction. In diseases of the vascular system we have changes of structure; in the latter not. It is necessary to make very clear the line which separates the two classes of disease, lest we fall into the common error of applying to both the remedial agents which are applicable to one only. The diseases originating in or involving the vascular system we treat locally by various agents—leeches, blisters, &c.; in diseases confined to the nervous system these local remedies are useless and even injurious, and we treat them through the constitution. In cases of tic do we derive benefit from leeches, or blisters, or from other form of depletive agents? Assuredly not.

Now the disease which forms the subject we have to consider belongs to the nervous and not to the vascular class; and I select from this variety that occasionally known under the term "Hysteria," than which no name can be more inappropriate or objectionable. It may well be doubted whether, except under very occasional circumstances, such a relation holds between the womb and this remarkable train of symptoms as to justify the employment of the term Hysteria. In the large majority of cases there is no connexion between them beyond that which the disease holds with the other organs of the body. In the name of a disease we are supposed to recognise its form and nature, whereas the term I have quoted conveys to the mind no distinct idea of either one or the other. And there is a positive objection to the resort to it in the fact that the word carries with it the association of a malady of small and insignificant dimensions, while the malady itself is of great magnitude. We associate with it the idea of "hysterics" and "vapours," as they were formerly called. I wish to raise your attention to the level of a great malady, and not of a trivial derangement of the hour. I remember a law case in which the counsel challenged a medical witness as to the name of the disease, and he replied, Hysteria. "Hysteria!" said the learned counsel, addressing the jury; "we all know what Hysteria means. My client has come into court to obtain compensation from a jury of his country for a permanent injury, by which all his prospects are blighted &c., &c., and the gentleman in the witness-box, with no sympathy for his misfortune, proclaims the disease to be a case of trumpery hysterics!" and, the jury, with rod in hand, let it fall heavily on the defendent's back. But there is a more solid objection than these, viz., that it is founded on a false pathology, in the employment of a term that conveys an impression of its source and nature founded in error. The disease consists in the local evidence of some irritation or derangement of one or the other of the nervous centres of the body, viz., the brain or the spinal cord—at least, such is the received pathology. But the subject is a very obscure one. We have no very definite idea of what we mean by "irritation." We all employ it, and so general is its use that I don't know how we can get on without it. "Irritation of the nervous centres" is a useful and not ill-sounding phrase, though somewhat mysterious, but it is no reflection on medical science that we can't explain all the phenomena of life; and as the term is somewhat wide in its application, and does not commit its employer to any very defined opinion on obscure matters, on which it is very difficult to form any opinion at all, I presume we shall retain it. One good reason that may be assigned for the persistent employment of the term Hysteria—a term we all know to be objectionable—is the difficulty of finding a substitute for it. We call the disease "local nervous irritation." It is "exalted nervous sensibility," but in naming a disease so definite as this we require a term equally pointed and definite with the thing itself. That we have not got. Sir B. Brodie says "I employ the term Hysteria because it is in common use, but the etymology is calculated to lead to great misapprehension."

Failing the name, let us look at the thing, and if it be so critical as I have assured you, let us attach to it the grave importance its frequency and its magnitude demand.

It may be asserted with truth that every part of the body may become, under provocation, the seat of an apparent disease that in reality does not exist—that it may and often does assume all the attributes of reality with an exactness of imitation which nothing short of careful and accurate diagnosis can distinguish from the real disease. You think this impossible. Surely you know a diseased knee-joint when you see it. You find severe pain, aggravated by the slightest movement. The temperature of the joint may be raised, and it is slightly swelled. You leech, you blister, you employ an iodine liniment (few cases escape it), you may even resort to issues, but the evil remains in spite of all your remedies, which have been applied to the wrong "system." It is the nervous, not the vascular, that is involved, but the nervous has imitated the vascular and deluded you, and led to the employment of false remedies, which have failed to reduce the pain or give mobility to the joint, and the general influence of which on the health of the patient cannot be said to have proved eminently serviceable.

The case, on more perfect investigation, proves to be one of local nervous irritation, or Hysteria. You think you will not be again deceived, but you are mistaken. A single error, corrected by the experience of another, will not teach you hysteria. You are consulted by a lady in reference to a daughter of 18 or

20 years of age, who has exhibited failing health for some time and now complains of her inability to walk in consequence of a pain in her back. You examine her, and discover that she suffers extremely on pressure over two or three of the lower dorsal vertebræ, or on any other of the twenty-four. You repeat the examination with the same result, and you make a report to the mother that her daughter has "spinal disease." The result of your opinion is two or more years' confinement to her couch, coupled with the usual allowance of restricted diet, alterative and other depletive medicines, leeches, blisters, and issues. Suppose these structures which you have declared to be the seat of organic disease to be examined under a microscope, what would you discover? Nothing. There is no disease whatever. As the nature of this malady dawns upon you, now awakening to a conviction of its frequency, you resolve to be more wary in your future diagnosis. You are now consulted by another young female patient on account of a tendency in one or more fingers to close in flexion. In the attempt to straighten them you cause intense pain, and if persisted in the consequences may be serious. Your patient appears in fair average health, and all her functions are regular and healthy; while the hand, for all ordinary purposes, is useless. Under the idea that she may have some chronic inflammation of the theca or of the palmar fascia, you treat it with the usual remedies. But your remedies produce no impression on the finger, which continues obstinately flexed as before; you adopt another principle of treatment founded upon a more correct diagnosis, and your patient recovers. These cases sound strange to your limited experience. You think they are rare, and brought forward from a distance and with an effort. By no means. They are cases of daily occurrence. If you could suddenly throw off that nebulous vision of vascular disease which years of bad pathology have impressed upon your judgment, you would see them in their true light. You may deem them to be exceptional. I assure you they constitute the rule of disease, and not the exception. *Real disease is the exception.* Speaking of one variety, and they have all characters in common, Sir B. Brodie, a man who rarely committed an error in diagnosis, says: "I do not hesitate to declare that, among the higher classes of society, at least four-fifths of the female patients who are commonly supposed to labour under diseases of the joints, labour under hysteria and nothing else." I would venture to enlarge this statement as regards the "upper classes," by including a large proportion of the lower; for much of my own experience of Hysteria has been obtained from the wards of St. Bartholomew's Hospital, and in reference to spinal affections in young persons, I unhesitatingly assert that real disease is not found in a greater proportion than one

case in twenty, and even this is a liberal allotment. Have you never experienced the difficulty of discovering an object floating in the air, such as a bird singing overhead, or an early star in the evening? When once the object becomes visible, the eye is readily adjusted to it, and when you look again in the right direction, it is the first object that strikes the eye.

And so with this class of diseases. They are not seen, because they are not looked for. If you will so focus your mental vision and endeavour to distinguish the minute texture of your cases, and look into and not at them, you will acknowledge the truth of the description, and you will adopt a sound principle of treatment that meets disease face to face with a direct instead of an oblique force, which far too generally claims the credit of a success for which nature alone is responsible.

I have selected above three varieties of this local hysteric affection. Let us consider them a little more in detail with a view to detect the fallacy which classes them under diseases of the first or vascular division, by which I mean an abnormal condition of the blood-vessels leading to changes of structure or altered relations, whether by suppuration, or ulceration, or fibrinous deposit, or local death of the tissues involved. In the first case the knee is the seat of pain. The subject is a young female. What evidence do we commonly look for when the joint is really diseased? We look first for a cause. Diseased joints don't occur without a palpable one, and particularly in young persons. There has been no violence, no fall or blow, to which to attribute it. Had there been, the nature of the disease is obvious enough. There is no considerable increase of heat, and if inflammation is present perceptible increase of heat is constant. There is no effusion into the joint; the form of the articulation is unchanged. The pain and the immobility or stiffness of the joint remain, notwithstanding your remedies. Local depletion relieves the pain of inflammation, but not of hysteria. But you persist in your principle, and the depletive treatment is continued, and thus months elapse—yes, even years. I was once told by a young lady that she had applied twenty-seven blisters to her knee-joint, from which she could not say she had derived any benefit. Now, it ought to be obvious that if a painful joint occurring in a young female without local cause is unaltered in form or size, and is free from heat or redness, and that the chief and almost the only symptom, that of pain, varies in degree at different times and is fluctuating in character, the disease is not of the inflammatory class; and if not it must be nervous, and you can't cure pain with leeches. You know that pain alone, which consists in an exalted nervous sensibility, does not constitute what we strictly understand by the term *disease*, although we apply it generally to any deviation from health, whether local or constitutional. At length the truth is brought home to you. You change your treatment by the substitution of local sedatives and general tonics, and your patient at once moves forward in the direction of recovery.

Take the second case. You have declared your opinion that this girl is the subject of disease of the spine upon the evidence of local pain produced by pressure of the fingers on the spinous processes of the vertebra. It has escaped your observation that this pain is equally severe whether pressure is slight or not, that, in fact, the degree of pain indicated by either writhing or exclamation holds no relation to the force of the pressure made. The slightest touch creates as much suffering as the greatest pressure of the hand, and *often more.* It is on this evidence alone you have founded your opinion of disease of the bony structure of the spinal column. It is on this evidence you have consigned this young lady to two years' confinement to her couch, to the loss of education, to restricted social and domestic intercourse with her family and friends, and to much moral and physical suffering. Now when you talk of disease of the spine, what do you mean? What *structure* is diseased, and what form of disease is present? Is it seated in the body, or in the processes of the vertebræ, or in the entire bone? and what description of disease has invaded the particular vertebra of the twenty-four? Is it inflammation, or caries, or necrosis? Caries, you will say; and you select this form because, and only because, you know the spinal column is the subject of carious disease under conditions favouring it. But there is this remarkable feature in carious disease of bone well worthy of notice—viz., that it is almost destitute of pain, that there exists no relation between the extent of the disease, which may be great, and the pain attendant on it. It is not like inflammation of bone, whether simple or severe, or necrosis. Presuming this statement true, can you in reason feel satisfied with the evidence of disease obtained by manual pressure? Then, again, where is the disease situated? If in the body of the vertebra, is it not almost absurd to suppose you can detect it by the slight pressure of the finger *on the summit of the spinous processes,* which are themselves rarely involved?

Fifty or sixty years ago a provincial surgeon of some note recommended the application of a hot sponge to the spine, with a view to detect disease of the bodies of the vertebræ. There was some excuse for ignorance on this subject at that time; there is none now. Of all the fallacies that cling to professional practice, of all the false doctrines [illegible] the pardonable ignorance of a former generation [illegible] on modern surgery, none can surpass that which [illegible] detect carious disease of the body of a vertebra by drawing the fingers down the spine

It is only not ludicrous because the consequences are so serious to the victim. It would be a bold assertion that such morbid changes in the spinal column cannot occur; but I do think humanity would be a gainer if all teachers concurred in asserting that they *could not*, so rare is the real disease, and so palpable to the eye when present. Suppose a young person in moderately good health, and occupied in daily exercise, complained of a pain in the condyle of the femur, without any other indication, should you be warranted in declaring she had serious disease of the bone? Look to the functions of this important column; how is it possible it can support the body in the upright posture if one or more of the component bones of the pillar are destroyed? And yet I have known many examples in which the subject of this imaginary disease has joined a party and danced for the whole evening. One wonders that such a person did not drop into pieces? For myself, I candidly declare that I have scarcely ever seen a case of true disease of this form. I can bear testimony to spinal affections and destruction of bone to any amount in psoas or lumbar abscess, or in angular curvature, or of damage done to the column by local injury; but to these suppositious cases, which exist only in the brain of the surgeon, I am a stranger, and if they exist otherwise than as rare examples of spinal disease I have much to learn. Have you ever seen a person recover from actual disease of the spine? I do not mean to infer that death inevitably follows, though that result is by no means uncommon; but I allude to recovery without some distortion or some permanent evidence of past disease. And yet you may be surprised when I assure you that all these young people recover sooner or later—sooner if the surgeon in attendance is familiar with hysteric affections, later if he is not. Thirty or forty years since these cases were, happily for our time, far more common than at present. At that date and for how many years anterior I know not, all the sea-side towns were crowded with young ladies between 17 and 25 years of age and beyond it who were confined to the horizontal posture, and were wheeled about on the shore in Bath chairs, on the supposition that they were the subjects of spinal disease. They were placed under much medical and dietetic discipline, not of the most invigorating character, and the large majority carried a pair of handsome issues in the back! Brighton, Worthing, Hastings, and other places on the south coast were largely tenanted by these unfortunate females, to which a moderate sprinkling of young gentlemen was added. What has become of all these cases? They appear to have vanished just in proportion as the eyes of the surgeon have opened to the absurdity of inferring that pain alone which locates itself with remarkable precision in hysteria on a given vertebra can indicate the presence of organic disease of the body of the bone without collateral evidence in its favour. When the spinal column is really diseased the case is obvious at a glance ; the health is degenerate, and the whole system proclaims to the eye of the surgeon the presence of a great evil. These examples are but a miserable mockery of the reality and a fraud on the judgment of the ignorant.

With regard to the third example, that of permanent flexion of the fingers, it is apparently so truly local an affection that there is some excuse for error, but only because hysteric affections are not half studied. When one or more of the fingers is permanently flexed from local causes, the seat of the disease will be found in the fascial structures of the hand or in the finger itself, or a joint may have been diseased or dislocated : but here there is no thickening, nor hardness, or other morbid change of structure. The finger is simply bent, and the attempt to straighten it is painful. The cause of this morbid condition of the flexor muscle is referred to its nervous centre placed in the cervical portion of the spinal cord.

It is not an easy task to select the class of constitutions most liable to hysteric diseases. Probably under certain conditions of impaired health the large proportion of the community would give evidence of its presence. Certainly it is uncommon in the lower class of males, and among those who occupy the beds in our public hospitals. We know, on the other hand, that it is most prevalent in the young female members of the higher and middle classes, of such as live a life of ease and luxury, those who have limited responsibilities in life, of no compelled occupation, and who have both time and inclination to indulge in the world's pleasures—persons easily excited to mental emotion, of sensitive feeling, often delicate and refined. Such are among the mental attributes of hysteria. But hysteric diseases are not confined to the young. I have seen many examples in females of 40 and 50. Do not imagine hysteria is a disease of persons of weak minds. It will often select for its victim a female member of a family exhibiting more than usual force and decision of character, of strong resolution, fearless of danger, bold riders, having plenty of what is termed *nerve*. If you tell such young people they are nervous, they take offence, because they misinterpret the meaning of the word, and so may you. And they may well misinterpret it, for, like the word "irritation," its popular meaning is both various and indefinite. It is essential that we attach a definite idea to this term in its different applications, and I must digress for a moment to endeavour to explain them.

I have already used it in one sense.

1st. The word nerve is used to express the mental condition of vigour, boldness, and resolution—as when a man's nerves are strung up to meet danger; so ladies are said to ride with more or less "nerve."

2. We employ it in its physical sense as a part of the general system of the nerves of the body.

3. We have the terms "nervous system," which may be not inappropriately defined as holding the same relation to the "system of nerves" in its physical sense that the physiology of a part holds to its anatomy. Yet this definition is imperfect, because the properties of the nerves of the cerebro-spinal system, with the small exception of those of specific sensibility, such as sight, smell, taste, &c., begin and end in the functions of motion and sensation. By the term "nervous system" we understand the general influence which the nerves in a physical sense exert on the constitution, the healthy or tonic condition of which is in a ratio with the combined force of the two systems —viz., the arterial, or circulation of blood, and the nervous. Health depends on the coincidence of these systems in perfect action. If the circulation fails in power, the consequence of this weakness falls on the nervous system, which is dependent on the circulation for its health and vigour. Failing an adequate supply of blood, this system is unstrung, and morbid sensations, endless in variety, take the place of real; and of all consequences hysteria is the most common. In the deficient supply of blood to the brain the faculties of the mind are involved. Confidence of strength gives place to fear, mental vigour to weakness and irresolution. Such indications are as untrue to the real character of the individual as the physical sensations are false and deceptive. Such is the nature of the large proportion of cases of persons who come into courts of law for compensation for what are erroneously deemed permanent injury, bringing with them headaches, spinal pains, tingling of the extremities, impaired vision, loss of memory, and many other symptoms of an unstrung nervous system—a series of grievances of the incurable nature of which an acute lawyer takes care to provide himself with ample testimony, and which will always be obtained so long as the diseases of the vascular system and their consequences monopolise a too prominent share of the attention of our profession. I have traced several of these persons in their after career, the large majority of whom entirely recover. I believe it is to the prevalence of error in the early management of these persons, who are almost invariably subjected to depletive treatment, and to the imperfect knowledge of nervous diseases which prevails in the profession, that large sums are awarded for injuries erroneously supposed to be permanent and incurable. Can it be reasonably expected that the truth will be brought home to the mind of a lawyer so long as our own opinions are yet uninstructed upon it? Sooner or later

their true nature will become established facts in the minds of our profession, and we shall no longer hear the painful discrepancies of opinion among medical men that now prevail. The light of improved knowledge will dissolve the daily mysteries which surround these cases in the form of supposed spinal concussions, partial paralysis, effusions into the theca vertebralis, thickening of the membranes of the brain, spinal cord, and lesions of this organ or that. These, as Dr. Sydenham declares, are but imitations and resemblances, and not realities, and that they deceive the multitude is undoubted. When real disease prevails there is no difference of opinion among medical men as to its existence.

It is a very interesting question, to investigate how far the functions of the mind are involved in hysteric disease, and how closely it is connected with it, whether the relation between them is direct and immediate, or remote. In cases of local pain, and also in the local contraction of muscles, arising either from an excessive action of one muscle, or from the loss of harmony of action with its antagonist, as in a permanently flexed forearm or finger, it seems difficult to identify the evil with that part of the brain which we believe to be the seat of mind. And yet an inquiry into the past history of such persons will often reveal the fact that they have been at one time or other the subjects of general or paroxysmal hysteria, or, in other words, that they have had hysteric fits ; and as mental emotion is more or less associated with this form of hysteria, it would appear not unreasonable to infer some remote relation between the mind and this variety of a disease apparently simply local in its nature. There is something in the mental development of these young persons very characteristic. They are quick and excitable, liable to sudden emotion without adequate cause. In very young persons the local disease may be developed before the mental character is fully matured, but advancing years will exhibit its peculiar features.

It is curious to observe the influence which the nervous system exerts on the daily condition of us all. When unstrung it preys upon ourselves. It is not in the varying force in our pulse, for that gauge is not sufficiently fine to detect the variations of health, that we can refer a consciousness of strength and vigour on one day that fails us on another. It is that our nervous system is more or less unstrung. There is a real illness and a factitious illness, and in this we observe the remarkable influence of mind in exercising a controlling power over the body. People without compulsory occupation who lead a life of both bodily and mental inactivity—people whose means are sufficiently ample to indulge in, and who can purchase the luxury of illness, the daily visit of the physician, and, not the least, the sympathy of friends—these real comforts come home to the hearts of those ornamental members of society who are living examples of an intense sensibility, either morbid or genuine, who can afford to be ill, and will not make the effort to be well. They are, in truth, well or ill, as you choose to take it, and they are only ill because they fail in mental effort, that mental resolution which is sufficiently powerful to rouse the dormant energies of the body and throw off the sensations of lassitude, of unreal fatigue and weariness of body and mind. A poor man cannot afford this indulgence, and so he throws the sensations aside by mental resolution.

There is a real fatigue and a nervous or unreal fatigue. A lady will tell you she was so tired that she could not walk another step. She thinks so, and without an adequate motive she cannot make the required effort. Give her the motive, such as the sudden illness of a relative or friend at a distance, and she will extend her walk to miles without effort or subsequent fatigue. How is this? It is that by a great motive acting through her mind she has called upon those dormant powers of her system which are possessed by all of us to be employed on critical occasions. Rarely, if ever, is the body subject to a degree of fatigue so great that an adequate motive will not obtain renewed exertion. When a lady tells you she can only venture on a walk of half a mile, you will understand that this effort is determined, by the ordinary, not the extraordinary motive. It is your duty as her medical attendant to place before her such inducements to a greater effort as shall call on the exercise of her dormant power, *the reserved fund* of physical strength, and she will walk four times the distance without fatigue. A poor man runs a race against time, and reaching the goal he drops from fatigue. Offer him at the moment £100 if he will run one hundred yards farther. He will accept the offer, run the required distance, and then drop. This is resolution acting on his muscular powers through his nervous system, screwed up by an extraordinary mental effort.

And this law of Nature is applicable to us all in our daily intercourse with the world. A man resolves to accomplish a certain amount of work on a given day, and he completes the task he has assigned himself by virtue of his resolution. Such resolution is eminently protective against fatigue.

A question arises to one's mind—Is Hysteria what is termed a specific disease, or is it the invariable result of a condition of health into which all persons pass in reduced states of bodily vigour, but only modified in degree? I presume it is associated with a peculiar organism common to man but not involving all, as some persons amenable to the influence of mesmerism pass readily into profound sleep, while others are entirely unaffected by it. It notoriously is far more common in women than in men, and in young persons from the age of 17 to 30, in the unmarried than in the married. We do not associate hysteric affections with persons of either sex who are characterized by vigour of mind, of strong will, of strength and firmness of character. Such persons may be reduced by protracted illness to a condition of weakness both bodily and mental, but they do not in their reduced strength, so far as I know, exhibit any of the peculiar features of hysteric affections. There is a remarkable form of hysteria which affords evidence on this subject. It is notorious that the sight of a person under an hysteric attack has a tendency to involve other hysteric persons around her. It has happened to me several times in my hospital career to witness the contagious, or rather the imitative, form of active or paroxysmal hysteria on a large scale. On one of these occasions, in a ward of 12 females, no less than 9 young women were affected at the same time. Several were so violent as to call for the assistance of sisters, nurses, and other servants of the establishment to restrain them; and inasmuch as a person under the influence of hysteria brings into action all the latent strength of her muscular frame, which is greatly in excess of her apparent strength, the services of these attendants were scarcely sufficient for the purpose—several requiring three or four strong men to prevent injury to their persons. The attack commences in the person of one girl, who may have been the subject of some trivial operation, or been brought under the immediate influence of the disease by mental emotion. No sooner is the condition of this patient observed by her fellow-patients than her influence is felt throughout the ward, and the second subject may become involved, occupying a bed at the remote end of the room, and thus it passes irregularly from bed to bed, each patient appearing to take the disease in the order of their constitutional liability. In the course of an hour, more or less, it subsides, and tranquillity is restored, but the evil only slumbers, and on the following day the same scene may recur—less violent, perhaps, but acted by the same persons as at first. Some of these patients, who were not affected to violence, were affected to tears and wept in silence, while some few were not implicated at all, nor did they show any tendency to sympathise with the disease. These curious attacks, though they appear to the subjects of them irresistible, are yet but the result of what has been termed a surrender, and might be prevented by an adequate motive. The mode adopted to arrest this curious malady consists in bringing these persons under the influence of some powerful mental emotion, and in making some strong and sudden impression on the mind through the medium of, probably, the most potent of all impressions, fear. They are not lost to consciousness, and for the moment, except in the intensity of their paroxysm, they will listen to the voice of authority. Sympathy and kindness, or tenderness of voice and manner, are worse than useless. They rather aggravate than mitigate the evil. Ridicule, to a woman of sensitive mind, is a powerful weapon, and will achieve something, but there is no emotion equal to fear, and a threat of personal chastisement will not necessarily be required to be carried into execution. On two of the occasions I have referred to, a few quarts of cold water suddenly thrown on the person of a chief delinquent instantly brought the ward to a state of reason and tranquillity. The disease succumbed to the indignity of the treatment. There can be no doubt, then, that a malady spreading by sympathy and cured by fear, has its origin in the mind. I think you will find on close inquiry that nearly all cases of paroxysmal hysteria originate in some form of mental excitement, and that of a depressing character, such as sorrow or disappointment. It is not the result of mere emotion. Joy, gladness of heart, or a sense of pleasure rarely produce it ; yet it is difficult to explain either its immediate or proximate origin in attacks occurring during sleep. Sometimes these patients suddenly awake from sleep with severe palpitations of the heart leading on to a direct attack. What can be their immediate cause ? There is no disease of the agents of circulation, or any suspected variation in the quantity of blood thrown upon the heart by which to explain it. Whence, then, the eccentric action of this organ ? Possibly some mental emotion in the form of a forgotten dream or some other occult mental operation which escapes cognisance, such as occurs in cases of somnambulism.

In these current remarks on general hysteria we must not lose sight of the subject taken in a surgical point of view. I have stated, both in this and in the last lecture. that under the condition of impaired health the nerves of a part of the body may become the subject of a deranged action by which, as Dr. Sydenham has declared, and we in our generation almost daily observe, so many symptoms of actual disease of that part may appear as to give the exact appearance or verisimilitude of local organic change of structure when such disease is entirely absent. There may be nothing apparent on a first inquiry to associate the case with hysteria, whether local or general. It is not necessarily nor commonly preceded by hysteric paroxysms. There may be no appearance of illness, no heat or undue excitement of the system, nothing, in fact, to connect it with hysteric disease ; yet it is nothing but local nervous exacerbation, and from the want of a better name we call it hysteria. You must not confound it with simple neuralgia ; and with still less reason, with epilepsy. It is not, however, always easy to draw a distinct line between neuralgia and hysteria, for both may have a constitutional origin, and be amenable to nearly the same treatment. In neuralgia, however, we have a more generally local and more persistent affection of a nerve. The disease appears to be limited to the nerve itself, the course of which may be traced by the pain, which is often excessive ; whereas in those cases of hysteria marked by local pain, the pain is general, involving the structures around in common with real disease of the part affected. In neuralgia the disease is placed on a recognised nerve, and a person is said to have neuralgia of a given nerve, such as the frontal, mental, or digital. In hysteria any locality may be affected without reference to the distribution of nerves ; while epilepsy is characterised by well-marked symptoms clearly of a cerebral origin. If you amputate a limb for hysteric pain, you throw the disease back on its nervous centre, and you kill your patient. In the early part of my hospital career I have seen this fact more than once exemplified. In such cases operative surgery is entirely out of place.

Now, before I proceed to illustrate these statements by reference to cases of which I have an ample supply, I wish to make a few remarks on the relation between local hysteria and the nervous centres—viz. the brain and spinal cord. Any facts that tend to throw even a gleam of light on the connexion between them must be interesting. I refer to the influence of anæsthetic agents, especially of opium and chloroform. In cases of hysteria marked by local pain, relief is given by the application of opium to the affected part—a fact which does not confirm the generally entertained opinion that the local affection is dependent on irritation of the nervous centre. Select a case of hysteric contraction of the muscles of a joint—the elbow or fingers. If you administer chloroform, the contraction of the muscles which may have existed for months and which has resisted repeated attempts to extend them, will now yield to a gentle effort of extension, and the limb is immediately restored to apparent repose. Supposing this morbid contraction of one or more muscles to be caused, as we believe, by irritation of the nervous centre, how does opium or chloroform affect it ? The effects of chloroform on the circulation are assuredly not in the direction of health, for it converts arterial into venous blood, or, at least, it gives to arterial blood the dark colour of venous, and we can hardly believe impaired circulation of a part of the body compatible with its improved function ; and yet the disease subsides. This "irritation of the nervous centre," as I told you, does not convey a very clear idea of the nature of the relation between the respective parts—viz., the seat of the disease, and its source or centre. Perhaps the nearest approach we can make to a solution of the difficulty is by saying that these two agents—opium and chloroform—suspend for the time the influence of both sensory and motor nerves, under which suspension the local pain or the erring muscle partakes of the general influence of the anæsthetic. To bring this morbid state of the muscle within the influence of the mind as its cause is almost of necessity to infer the local evil to be wilful; but if it were so, the state of unconsciousness during sleep would remove it, which it does not, for the contraction is constant by night and day, while the specific influence of the chloroform suspends the disease, if it does not cure it, and the renewed, though partial, contraction of the muscle is now prevented by mechanical agency. The remarkable circumstance consists in this, that a disease of long standing which incapacitates for exercise and occupation is removed in a few minutes by the agency of chloroform, and the patient placed at once on the high road to recovery. Is this curious fact confirmatory or otherwise of the origin of the disease in the nervous centre?

Unfortunately hysteric persons have no exemption from real disease, and when the two are found in combination a difficulty in diagnosis will frequently occur to test the pathological knowledge of the surgeon. The local disease is accompanied by symptoms of an eccentric character that do not legitimately belong to it. Local pains are aggravated in the active stages, and do not subside in a degree proportionate to the local improvement. A small malady such as a sprained wrist or ankle, is magnified into a large one. The constitutional symptoms take the direction of hysteria instead of fever. The vascular system indicated by the state of the pulse, the skin, &c., is less involved than the nervous, and months will often expire before recovery is complete. To a surgeon not familiar with hysteric disease, who practices his profession with reference to one only of the two systems of which the body is composed, these cases will always be obscure and difficult of management. When an injury occurs to the person of a young female, and to many others neither young nor female, hysteric symptoms are almost certain to develope themselves in some form or degree before recovery is complete.

[It is an undisputable fact that any part of the body can be the seat of such local derangement, either of the nerves of sensation or motion, as shall actually represent disease in that part, when no disease, properly so called, exists.]

Cases of real hysteria may be reckoned by hundreds in the practice of any one surgeon. The more common seats are the female breast, the side of the trunk under the ribs, the whole spinal region from the atlas to the sacrum; any joint, but especially the knee; the stomach, the bladder, and the ovaries;

the muscular system of the extremities, indicated by spasm or permanent contraction; and the muscles of the larynx. But no part of the frame has exemption from liability, so far as I am aware.

I will first quote a very simple case of hysteria, the evidence of which is immediate and the attack transient. In some slight forms the patient loses all command over the voice, which suddenly sinks to an almost inaudible whisper, without any other accompanying symptom. I have seen many examples, but that I give occurred under my own observation, as it has probably in some form occurred under that of others, for it is as old as history. The subject was a young lady of about 20, as Sir B. Brodie observes, of pale complexion, and having cold hands and feet. While I was engaged in conversation relative to her health, I somewhat imprudently remarked that a mouse was running about under the table at the end of the room. She uttered an exclamation of alarm, and in an instant so entirely lost the power of audible speech that I was obliged to approach her and to put my ear close, to hear her. The ferocious cause of the mischief having paid the penalty of its intrusion by the loss of all it possessed on earth, the lady in the course of an hour recovered her voice. Had this person been in sound and vigorous health, she would probably have sustained the shock to her nervous system with less derangement of it. The case is interesting as showing the sudden influence of the mind on a particular nerve in the general system. Ammonia, chloric ether, henbane, &c., quickly administered, would probably shorten the attack. and for which agents brandy is a good substitute.

Cases of hysteric affections of the breast occur in young persons from 16 to 20. They are associated with a disturbed condition of the genital system, but not especially with the functions of the uterus itself. Although the catamenia is often deranged and defective, it is not necessarily so. The general system is at fault, indicated by a low circulation—frequently a chlorotic aspect, failing appetite, languor, and indisposition to any form of active bodily or mental effort. In the cases I have seen, the breast has been small and soft. The disease consists of simple pain in the organ, one or both, but more severe in one than the other. It is most active at the catamenial periods. The breast is unaltered in form and substance. The evidence of local inflammation is entirely absent. Leeches, or other forms of local depletion, give no relief. Blisters and irritating plasters and ointments answer no useful purpose. The degree of pain varies with the condition of the health. For a period it may almost cease—a period co-existent with a change of air and occupation, or a residence of a month or two at the sea-side, but the pain relapses on the return of the subject to the ordinary habits of life. The pain is dull and aching, and very unlike the smarting and shooting pain which accompanies abscess or the more formidable diseases.

With this history who will doubt the constitutional nature of the malady or the efficacy of such remedies as tend to change weakness for strength, to promote appetite, to keep the circulation in action by frequent exercise, taken at least twice daily?

Of examples of hysteric pain situated under the ribs, more commonly on the left side, it is needless to quote individual cases—they are so common. From some cause not very apparent, they are, however, seen less frequently than formerly. I attended, some years ago, a young married lady, the mother of three or four children, the daughter of a medical man of large experience, by whose direction she had been cupped about fifteen times over the seat of pain. The malady prevailed in her system in its active form during many years, and she was not free from it when I saw her at the age of 30. This treatment, which included the local application of leeches by the hundred, and blisters, the sum of which might be calculated by the square yard, while it gave no permanent relief, has left its mark in more senses than one on the person and constitution of this lady for life. At all events, her excellent parent has the merit of perseverance, if not of discrimination.

Among the cases of sympathetic or imitative forms of hysteria which I have already quoted, two of these females exhibited the scars of similar local treatment for supposed organic disease under the ribs, and I have seen many others. I need hardly tell you that this is a constitutional and not a local infirmity, and must be treated accordingly, *or not treated at all.* What structure or organ occupying this region on the left side, under the lower ribs, can be supposed the seat of this pain? It is deep-seated, and therefore the abdominal muscles are beyond suspicion. Is it the colon, or the spleen, or the base of the left lung, or the diaphragm? Whichever structure is involved, if any, rely upon it the essence of the malady is seated in the nerves, and the nerves only. If it were organic disease, its nature would become in time palpable. There is this important distinction between the two affections—that organic disease has a crisis, and nervous affections for the most part have none.

Spinal hysteric affections are, perhaps, of all hysteric maladies of the most common occurrence—happily they are becoming somewhat notorious. It is in the records of pathology, no doubt, that a young female may be the subject of real spinal disease; but where are the cases to be found? You may pass through life and not see two. And while I state this opinion your minds may possibly revert to some case you have already attended which you think exceptional. Look more closely into it, and you will detect your error. In forming an opinion on any given case on which you may be consulted hereafter, you had better make a starting-point from the knowledge of this fact, that nothing in pathology is more improbable than that a young lady should be the subject of organic disease of the spinal column. Well, a case presents itself for your opinion. A young female, in any class of life, in apparent health, pale or florid in complexion, bearing in her appearance no indication of disease, complains of pain in the back. This pain may be announced without surgical inquiry, or may be detected only on examination. The spine is exposed while the person is placed in bed. Pressure is made by one or more fingers on the spinous processes of the vertebra, beginning with the atlas. On reaching perhaps the last dorsal or first or second lumbar vertebra, the girl utters an exclamation of pain, and she instantly shrinks from the pressure. The examination is renewed again and again with the same result. Twenty-three vertebræ admit of pressure through their spinous processes without causing suffering. Pressure on the particular one, or perhaps two, causes instant and often severe pain. Who ever heard of real disease attacking one or even two vertebræ only?

I have already told you the probable result of this inquiry, but you, I trust, will not be deluded. Be assured to the extent almost of certainty that there is no organic disease, either of bone or of any other texture. I attended a girl in St. Bartholomew's Hospital of about 20 years of age. She had the appearance of a strong and healthy person, and there was nothing in her aspect to indicate that she was the subject of disease. Before I reached her bedside the house-surgeon informed me she was the subject of "spinal disease," and I smiled at his credulity. To the students around I said: "If on examing this girl she makes an exclamation of pain and shrinks from the pressure of my hand, rely on it she has no disease whatever, and that her case is one of simple hysteria." On reaching the first and second lumbar vertebræ she uttered an expression of severe pain, and nearly threw herself out of bed. The diagnosis was confirmed, and she was treated for a nervous, and not a real disease. Extract of opium dissolved in soap liniment was rubbed on the spine for a few days, and then the opium was omitted, and the back generally rubbed by the hand twice daily with some force of pressure. She was ordered valerian, bark, iron, and a full diet, with wine. Her recovery occupied one month.

I was consulted in the year 1862 on the case of a young lady of about 24 years of age. She had had "spinal disease" for several years, and many surgeons of more or less eminence had been consulted on her "very remarkable case." Her aspect was that of a healthy person. She was inclined to be stout, and exhibited no indication of serious disease, or indeed of disease of any kind. During five years her back had been most liberally cupped, leeched, blistered, and embrocated without benefit. I was informed that the pain had occasionally intermitted, that her condition had improved for a time and then relapsed, and that although nearly the entire five years had been passed in her chamber and in the horizontal posture, yet that occasionally she would join her family and seek relief from the monotony of her life in the gaiety of the ball-room, where she forgot her diseased spine and all its attendant miseries, and danced for hours with life and animation. I examined her back with more than usual care. The pain, always true to its own locality, occupied the second lumbar vertebra, and always returned on the pressure of my finger on that particular spot. Occupying her attention by conversation, I gradually subjected the whole back first to gentle, and then to severe, pressure. With both hands I grasped the trunk, and moved it forcibly in all directions without creating any sensation of pain. I then passed the flat of my hand rapidly down the spine, employing not pointed, but obtuse pressure over the whole surface, and thus satisfied myself that there was no disease. After the interval of a few minutes, pointed pressure on the second lumbar vertebra produced the same symptoms as at first. On examining the surface I observed the mark of a cicatrix of about three inches in length running along the side of the affected vertebra, and on inquiry I learnt that one surgeon whom the family had consulted had deemed it necessary to look within and below the surface under the supposition that there might possibly be a tumour or some morbid growth, the removal of which would be conducive to her recovery! Nothing, however, was found, and the excision of a small portion, I presume of the erector spinæ muscle, afforded no permanent relief—at least no benefit had arisen from the operation at the expiration of many months, when I was requested to see her. It struck me that this was carrying the experiment of operative surgery rather far, but I did not make any remark to that effect at the time. I certainly made an inquiry as to the product of the operation, and the father of the young lady told me that he was shown something, but he was not competent to state exactly what it was. On discussing the nature of the case with the family and the attending surgeon, I expressed my conviction of the hysteric nature of the disease, and that she was capable of exertion could she be induced to attempt it. I saw at once that I had failed to convey my own convictions to the family, that my opinion was not satisfactory, and that in the judgment of the lady's father, a very sensible person, the opinion of one man could not outweigh that of the many, and that the testimony of the many was the safer guide. The patient returned to her couch, on which she may be now reposing for ought I know to the contrary, for I saw her but once. I had, however, the satisfaction to hear the medical man say as I left the house, "I believe your view of the case is the only true one."

As treatment by means of issues was formerly in great resort, and is yet far from being abandoned as a means of checking the progress of carious disease in the vetebræ, it is worth considering for a moment the principle of its action. To control one disease you make another, which is supposed to act as a drain in carrying off the morbid actions of the original disease by derivation, or counter-irritation as it is termed. An issue is an ulcer, secreting matter, and drawing more or less on the powers of the constitution. An ulcer is a disease. All disease exercises a depressing, not an invigorating, influence on the system. The sum total, then, is increase, not diminution, of the evil. The morbid condition of true spinal affections is *caries* or crumbling of bone, not inflammation. Is it probable that a pair of secreting ulcers can tend to restore bone that is lost? Will the capillaries be more likely to secrete material to be converted into healthy bone within the body because you have made an ulcer outside? The actions going on within are those of deficiency, and not of excess. Here comes in again the old doctrine of inflammation. The operation of an issue is equivalent to that of the lancet, and in these days that instrument has become absolete in the hands of all sensible and thinking men. I acknowledge with all regret, in looking back at the early part of my own professional career, to have frequently committed this error in treatment, and I willingly make retribution to another generation by declaring my conviction of the entire futility of an issue in this description of disease to answer any useful purpose.

While on a visit at the house of a friend in the country, I was requested to see one of his daughters who had been confined to her room for fifteen months in consequence, as I was told, of diseased spine. She was 21 years of age. Her countenance was pale, but not unhealthy. She had been condemned by a court of surgeons to a long confinement to the horizontal position, and she bore the judgment against her with resignation and humility. From the appliances around her, and the general arrangements of the room, it was obvious that the siege was to be long and vigorously maintained. My visit was not a professional one, and I did not propose to myself at the time to discuss the subject of her disease. Accident brought me into contact with her medical attendant, and in the course of conversation with him some features of her case were mentioned, which appeared not very consistent with real disease of the vertebræ. We examined her carefully, and the consultation which ensued terminated in the proposal that his patient should change the horizontal posture for that of an inclined plane. In a week she sat upright in an easy chair, and within a month she joined the family circle, entered into all their pursuits, and could ride any reasonable distance on horseback without fatigue.

I see no advantage in multiplying these painful examples of hysteric disease. They have all general characters in common, and are amenable to the same principles of treatment. And so with cases of supposed injury from railway accidents. Again and again have I heard medical men, physicians, surgeons, and general practitioners come into courts of law and state their opinion that the plaintiff had sustained grievous and probably permanent bodily detriment to the spinal column, on the evidence of pain produced by pressure of the finger on one or more of the spinous processes—evidence far more than counterbalanced by the fact that these deluded persons have walked unaided into court, and have stood or sat in the witness-box for three-quarters of an hour while under examination.

No evidence of their reputed symptoms is obtainable either through the eye or the touch of the surgeon. There is nothing palpable, nothing organic. You take the assertions of your patient on trust, you identify yourself with his case; you place an object before him, and he declares he cannot see it; you refer to an occurrence that happened last week, he declares he does not remember it. He suffers incessant pain in his back; he staggers in his walk, occasionally coming to a harmless fall; he has convulsive twitchings in his leg, occurring chiefly in bed, which he says he cannot control. He passes blood in his urine, which always escapes the notice of others, for it is invariably passed at the water-closet, and at no other time; but on inquiry you will find that this afflicted person can walk four or five miles; that as regards his vision his iris acts well, and the ophthalmoscope detects nothing; his appetite for food is sufficient for perfect nutrition. And let me ask you finally whether on these conditions it is more than remotely possible such a person can be the subject of any serious organic disease. But presuming on the possibility of such contradictory evidence occurring in a single and exceptional case of real disease, will your credulity reach so far as to admit of their frequent occurrence? To you such cases will be presented singly and individually, but they are brought into courts of law in multitudes.

One of such cases I will give you as an example:—A man without property or profession brought an action against a railway company for injury to his spine. This statement, on the face of it, is an absurdity. How can a man without property bring an action at law? Well, he applies to a lawyer, who undertakes the case on his behalf, with a certain compact and understanding as to the question of future payment. Thus the lawyer becomes the plaintiff, and the plaintiff the witness in his own case. The man's injury was made out to the entire satisfaction of the jury, and very heavy damages were awarded by them, coupled with severe comments on the negligence of the railway directors.

It was positively known, at the time, by several persons engaged in the action, among whom was a detective officer, that within a few days of the trial the plaintiff, or the witness, whichever you please to term him, *had walked a match* against another man! Yet this man was declared on authority to have sustained a permanent injury of his spinal column!

With respect to hysteric affections of joints, knee cases, &c., they are in truth as common as Sir B. Brodie has declared them to be, and I thoroughly corroborate all he has said on the subject of this most important and interesting disease. Three-fourths of all knee cases in the upper classes of society, says this great authority, are not cases of inflammation, though they appear so. There is no organic disease whatever in the joint. They are cases of local pain, originating in impaired health. They are not amenable to treatment for inflammation and its consequences. Your liability to an error in diagnosis is just in proportion to the supposed infrequency of local nervous, as compared with vascular, derangement. The knee is by far the most frequent seat of these affections, and the cases are found among young women not in the lower class of life—but even this class is not exempt. You will find, on the occasion of your first visit, the patient walking lame. This lameness has existed for several days, probably weeks, before attention has been attracted to it, and has come on very gradually. The joint is stiff—not that it won't bend, but the movement is painful. There may be some increased heat in the joint, when compared with that of the opposite limb, but not much in degree. The knee is slightly swollen. If you see the case after treatment has commenced—*i. e.* after the repeated application of leeches, blisters, and tincture of iodine (the almost universal agent in difficulty)—the swelling will be palpable, and the outline of the joint has undergone a change. As the case progresses, the lameness increases, but the aspect of the joint remains as in the first stage—neither the swelling nor the heat increasing in the same proportion. In this condition the limb may remain for months, or even for years, subject to the same treatment, without improvement. One feature in this case ought to have struck you as worthy of notice—viz., that so many months have passed without organic change; the joint is neither stiffer, larger, nor hotter than it was in the early stage of the treatment. I say it ought to have struck you. Perhaps it has not! The aspect of this lady is that of unhealth. She has become pale, partly from depletion, partly from loss of exercise. Her pulse is weak, her appetite bad, and catamenia, as a rule, defective. You fear to give tonics and alcohol, lest you aggravate the supposed local inflammation.

Having exhausted the negatives in treatment, you now venture on an onward step, and you give bitter infusions, gentian, cascarilla, with ammonia and ether. But you are still behind the necessities of the case; you have adopted from the beginning a false diagnosis, and the difficulty is how to get back to the right groove. There is only one course: begin afresh, and treat your case on a different principle; convince yourselves that nerves may go wrong as well as arteries and capillaries, and as you treat excessive action, rightly or wrongly, in the blood-vessels by local depletion, so apply such remedies as check excessive action of nerves in the form of opium, belladonna, chloroform, &c. Build up the health by increasing the force of the circulation. The agents are a thoroughly nutritious diet, wine frequently in small quantities, tincture of bark, iron, fresh sea air, change of locality and associations, agreeable mental occupation. Assure your patient she has no real disease, but the semblance only. Leave the functions of the alimentary canal to take care of themselves. The constipation incidental to a low innutritious diet and an inactive life will subside under the influence of a nutritious one; improved health will restore its functions. There is no real harm in a day's constipation; it is sometimes a good. At all events leave the bowels alone. With regard to the joint, rub in some blue ointment and extract of opium, in the proportion of one-third of the latter, and roll it firmly with a flannel bandage. Encourage moderate daily exercise on a level ground, on a carpet, or on a lawn. If the case is chronic don't be disappointed if the progress be yet protracted to weeks. The pain and the stiffness may subside very slowly by virtue of their long possession by the joint; but you are in the right path, and rely upon it your patient's recovery will justify the sound principle of your treatment.

In the course of last year I was consulted by the family of a young lady of 18 years of age living at a distance from London relative to an affection of the knee from which she had been suffering for a period of ten months. The joint was stiff and painful; she moved about on crutches; there was no considerable amount of heat, and what alteration existed in the form and outline of the knee was due to the activity of the past treatment; the tissues had lost their natural softness and flexibility; the joint had been repeatedly leeched and blistered, and subjected to the application of liniments in variety of colour and composition; an issue had been made on the inner side of the patella, which, judging from the cicatrix it left behind, had not been a small one, and the curative influence of which had not been discoverable during four months, at the expiration of which nature was allowed to heal it.

I considered this a case of hysteria on the following evidence. The subject was a young lady of an hysteric age. She had sustained no sudden injury to the joint, neither blow, nor fall, nor sprain. The malady was gradual and spontaneous. Had the disease been of the inflammatory class, the remedies would have probably long since cured it. There was no appearance of disorganisation otherwise than integumental. The pain was generally aggravated at the catamenial periods. Bending the joint afforded no evidence of disease within it, no grating or roughness of the cartilaginous surfaces. The pain varied greatly in intensity at different periods. This evidence was sufficient, and. to my judgment, conclusive. I strapped up her joint in an opium plaster. She took bark and iron and wine, and in a fortnight began to walk about without her crutches; but two months had elapsed before her recovery.

Many years ago, when I was less familiar with hysteric affections, I attended the case of a young lady of 19 in conjunction with Mr. Stanley. We both deemed the disease to belong to the class of inflammation, and conjointly adopted the usual remedies so indiscriminately resorted to in all painful affections of joints. Many weeks elapsed without improvement, and I remember that we discussed with some anxiety the probable issue in abscess, destruction of ligaments, absorption of cartilage, and ultimate amputation of the limb.

One day my patient informed me that her sister was going to be married, and that, cost what it might, she had made up her mind to attend the wedding. At this proposal I shuddered. Having expatiated, to no purpose, on the probable consequences of so rash an act, with all the force of language I could command, I determined to give stability to the joint for the occasion, and I strapped it up firmly with adhesive plaster. On the following day I visited her. She told me she had stood throughout the whole ceremony, had joined the party at the breakfast, and retured home without pain or discomfort in the joint. Within a week her recovery may be said to have been complete.

This case first brought home to my mind the nature and the frequency of hysteric disease.

[Mr. Skey, after some preliminary remarks, proceeds to give a few examples of the less common forms of hysterical disease.]

A young woman, aged 24, was admitted into one of my wards at the Hospital who was the subject of difficult deglutition. She was a very respectable person in character and position, and had been for several years a much-esteemed servant in a good family, and was a young woman of some education. For two months previous to her admission she had complained of difficulty of swallowing her food. As the evil appeared to increase, the family medical attendant was consulted, by whom she was treated for a stricture of the œsophagus. One or more consultations were held on her case, and the œsophagus examined carefully by means of probangs and bougies. These instruments, however, failed to pass a given spot corresponding with the base of the neck, or about one-third from the commencement of the tube. She had no local pain whatever. As the obstruction increased, nothing but semi-liquid food passed into her stomach, and this was only effected with a difficult and painful effort. She became emaciated by reason of defective nutrition, and at the time of her admission into the hospital was weak and somewhat attenuated in form. For many weeks she had taken no description of solid food, and even liquids passed the obstruction with difficulty. The malady now assumed a serious form, and with a view to additional advice she was sent to the hospital. The case was reported to me on her arrival as that of "stricture of the œsophagus," and I will tell you the preliminary train of thought that passed rapidly through my mind before I opened my lips to the students on the subject. Real stricture of the œsophagus is at all times a rare disease. When present it is almost invariably a cancerous affection. Cancer is a very rare disease at this young woman's age. For so serious a malady as cancer she does not look ill enough; for though the presence of cancer of the breast occurring at a later period of life by twenty years may be for a time compatible with fair average health, cancer of the œsophagus stamps the constitution early. She is of an hysteric age, and though thin, she does not look absolutely ill. There is nothing of disease in her aspect, nothing that may not be referred simply to defective nutrition. The history of her case was given me by her medical attendant, who was present on the occasion, and I had no hesitation in recording her disease as that of "hysteria." I declined the use of a probang or bougie which lay on the table before me, and I simply said, "We will endeavour to remove the obstruction without the aid of instruments of any kind." Her catamenial discharge had been regular throughout. I ordered her bark, iron, valerian, wine, milk with brandy—each to be given in the largest quantities at the shortest intervals *consistent with reason and moderation*; three times in twenty-four hours, enemata of thick soup with an ounce of brandy. These various agents were absorbed into her system with the greatest advantage to her health. Within a week she could swallow finely minced animal food, and in three weeks she ate a portion of a rump steak without difficulty, and was, in fact, convalescent. She was in high spirits at her recovery, and the only vexation she suffered arose from my refusal to pass a probang down her throat before she left the hospital. This I peremptorily declined to do, assuring her that a probang of rump steak was a far more efficient test of her recovery than any instrument in surgery bearing that name.

A young lady of 18, and of slight form, was brought to me from the country with gastrodynia. For upwards of a year she had suffered intolerable pain in the stomach on taking food of any description. She was much emaciated. and her pulse extremely feeble. Neither trouble nor expense had been spared in her treatment. Her family had consulted medical men of eminence in more than one metropolis, but the severity of the pain continued in spite of treatment. On entering the drawing-room, I heard the sound of suffering from an adjoining room, and I was told that my future patient was paying the penalty of a slight meal of arrowroot, of which she had swallowed a few table-spoonsfuls only. Having intruded myself into the room somewhat unexpectedly by its occupants, I saw this young lady in a condition of great suffering, in the upright position, leaning her head upon her mother's shoulder and sobbing painfully. In the course of a quarter of an hour I had obtained some insight into her case, but I could not fail to observe that the mother habitually interposed replies to questions addressed to the daughter, and I explained to her the necessity of my obtaining the answers to my inquiries direct from her daughter. At my request she left the room. Up to that time I had but an imperfect knowledge of the case, but I then led the conversation to subjects which carried the girl away from her malady and all its associations. I spoke of her home and the scenery around it, of which I described the general characters, and enlarged on the beauty of the neighbourhood, the lovely rides and excursions, &c., and in all of which I was tolerably successful, considering that at that time I had never seen it. However, the description was sufficiently accurate for my purpose, for it succeeded in distracting the young lady's attention from her suffering, and during the few minutes which this conversation occupied she was to all appearance entirely free from pain. She talked freely and cheerfully, and not the slightest reference was made by either of us to her former suffering. I then changed the subject by saying, "I think your pain has flown away," when she immediately resumed her crying fit and sobbed as before. She assured me she was in great pain, and that the sensation had been but suppressed. That this was a case of severe hysteria was highly probable even had I gone no further with the evidence, relative to which the following thoughts occurred to my mind. What could be the nature of this pain if not hysteric? I was told by her family that pain followed the act of deglutition, not remotely or at an interval of one or more hours, but almost as immediately as the food could reach the stomach. This could not be dyspeptic or common gastrodynia, which waits on the process of digestion, and rarely occurs within a period of two hours of taking food. There is one disease only of the stomach in which pain follows the admission of food into it, and that occasionally only—viz., cancer. Was it probable, or scarcely more than possible, that this girl of 18 could have been for so many months the subject of undetected cancer of the stomach? If cancer, could the attendant pain, so severe as it appeared at the commencement of my visit, be suspended by conversation? It was neither gastrodynia, the result of indigestion, nor cancer; and if not, what remains behind to elucidate the case? It could be nothing but hysteria, and hysteria alone could solve the mystery. But she had been treated for gastrodynia and treated for cancer, but she had not been under treatment for hysteria, simply because these varieties of local hysteria have never yet fixed themselves on the attention of the profession. To tell a practitioner of the old school that a young lady was the subject of hysteria of the stomach would be to raise a smile at your expense.

It would be an unprofitable employment of our time were I to enlarge on the subject of the previous treatment. The remedies included, in different proportions and in varying doses, ammonia and other alkalies, under the mistaken supposition of acid secretions; opium in various forms, creosote, bismuth in small and large quantities mineral acids, &c., &c.

How difficult it is to ascertain beyond all question the real value of many drugs in daily use amongst us! Although, in common with others, I have frequently employed the trisnitrate of bismuth, I have to this hour no conviction of its utility. This case did not terminate so satisfactorily as I hoped. That it was a case of hysteria admits of no doubt, but I had difficulties to contend with in the domestic management of the young lady. Although her symptoms remitted greatly under the use of remedies, she did not entirely recover in the brief period of three or four weeks during which she was under my care. I gave her small doses of ferrocitrate of quinine, two or three times a day a wineglass of port wine boiled with rice, and I ordered a plaster of the fluid extract of opium to be applied on the epigastrium. If, coupled with these remedies, I could have separated the girl from her family, whose sympathies with her were far too redundant for her benefit, I think she might have been cured in one month. In a case of this kind good domestic moral treatment is indispensable to success.

Hysteric affections of the ovaries are extremely common. Several of such examples I attended with my late friend Dr. Rigby. The cases I have seen have occurred in young females of between 20 and 25 years of age. They are characterised by deep-seated aching pain in the region of the ovary, about two inches above the crural arch. My own observation would lead me to say that the right organ is more frequently affected than the left, but this is probably accidental. Like other hysteric affections, its severity varies with the constitutional health, mental and bodily. It yields but slowly to remedies, and though mitigated, it often returns at longer or shorter intervals. I have applied opium locally with advantage, but an entire change of air, scene, and occupation, combined with tonic treatment, are indispensable to recovery.—*Medical Times and Gazette, Aug.* 4 *and* 25; *Sept.* 22 *and Oct.* 13, 1866, *pp.* 109, 191, 309, *and* 385.

13.—ON THE SECONDARY COMPLICATIONS OF PAINFUL AFFECTIONS OF THE FIFTH NERVE.

By Dr. Francis E. Anstie, Senior Assistant-Physician to the Westminster Hospital.

It is to M. Notta that we owe the first scientifically arranged treatise on the subject of those complications which affect the organs of sense to which the branches of the fifth nerve are distributed. In a series of papers published in 1854 this author gives an elaborate analysis of no less than 123 cases of facial neuralgia, from which we gather the following important facts. In the first place, as regards the organs of special sense. The retina was completely or almost completely paralysed in 10 cases, and in 9 others vision was interfered with, partly, probably, from impaired function of the retina, put partly also from dilatation of the pupil, or other functional derangement independent of the optic nerve. The sense of hearing was said to be impaired in 4 cases. The sense of taste was perverted in one case, and abolished in another. Next, as regards modific-ations of secretion. Lachrymation was observed in 61 cases, or nearly half the total number. The secretion of the nasal mucous membrane was repressed in one case, in 10 others it was increased, upon the affected side. Salivation was observed in 14 cases. Unilateral sweating is spoken of more doubtfully, but is said to be probably present in some cases. Elevation of temperature is said to be present on the affected side of the head in an undetermined but probably a considerable number of cases. In 8 cases there was decided unilateral redness of the face, and five times this was attended with noticeable *tumefaction.* In one case the redness and the tumefaction occurred, not only in the accesses of neuralgic pain, but also persisted in the intervals, and, in fact, were accompanied by a definite hypertrophy of all the tissues. In the conjunctiva vascular dilatation was far more frequent, occurring in as many as 34 cases. As regards *lesions of nutrition*, hypertrophy of the tissues of the affected side of the face was noted in 4 cases, in 2 the hair was noticed to be hypertrophied at the end, and several others are referred to in which the hair turned grey, or fell. The tongue also is reported to have been greatly tumefied in one case. As regards *muscular affections;* there were noticed convulsive contractions of the muscles of the affected side in no less than 52 cases. Of these 13 are noted as instances of contractions of the muscles of the lip and nostril, 10 as tremor of the eyelid; in a great number of cases many muscles were simultaneously affected. *Permanent tonic spasm* was observed in the eyelids in 4 cases (when it was not due to photophobia), in 4 cases in the muscles of mastication, and in one instance in the muscle of the external ear. *Paralysis of the motor oculi*, inducing prolapse of the upper eyelid, was noted in 6 cases, and in the half of these cases there was also external strabismus. In 2 cases the facial muscles were paralysed, obviously not from an original affection of the portio dura, but from severe neuralgia of the auriculo-temporal branch of the fifth. As regards the pupil, it was dilated in 3 cases, and contracted in 2 others, without any simultaneous affection of sight; in 3 other instances it was dilated, coincidently with considerable diminution of visual power. Finally, with regard to common sensibility, M. Notta reports 3 cases, in which anæsthesia was observed. With regard to the hyperæsthesia so often noticed, he justly observes that it only occurs in the more advanced stages of the disease.

I have analysed the results obtained by M. Notta somewhat at length, because, with regard to the majority of the complications of facial neuralgia observed by him, my experience affords ground for the belief that his estimate of their proportional frequency is very correct, and I therefore preferred to give you his figures because they are drawn from a larger number of cases than my own. But there are certain especial complications, which are either not noticed or are noticed so cursorily by M. Notta, as well as by Valleix, Romberg, and all the other writers whom I have studied, that I cannot refrain from citing my own experience.

The first group of cases which have thus particularly attracted my attention are distinguished by the presence of remarkable affections of the vaso-motor fibres.

The most important of these is facial erysipelas. Some years ago I was much surprised at observing, in a woman 32 years of age, an out-patient of the Chelsea Dispensary, a most acute attack of unilateral erysipelas of the face and head, supervening after she had suffered for two or three weeks from very severe and frequently recurring attacks of neuralgia, which affected all three branches of the fifth, but was most violent in the branches of the ophthalmic division. On the occurrence of the erysipelas the acute pain subsided, but the most intense tenderness remained for some days, and pressure anywhere in the track of a considerable branch of the nerve would re-excite a momentary spasm of neuralgic pain. Since that time I have been constantly on the look out for similar cases, and I have made some observations for which I was not at all prepared. In some cases I have actually seen neuralgia of the fifth terminate in well-marked erysipelas of a strictly unilateral character; in four of these cases it was limited to the side of the nose, the infraorbital and frontal regions; in all of these the neuralgic pain had been limited to the supraorbital and superior maxillary divisions of the nerve. The last of these patients was a young man who came under my care at the Westminster Hospital a few weeks since, suffering from commencing erysipelas of the left side of the nose, forehead, and cheek. The circumstance which principally attracted my attention was a convulsive action of the left eyelid, such as is often observed in neuralgic affections. On questioning the patient, a very clear history of a previous severe neuralgic attack was elicited, and the existence of extreme tenderness to pressure in two limited spots, corresponding to the supraorbital notch and the infraorbital foramen, was ascertained; there was also intense photophobia, lachrymation, and conjunctival congestion.

But the facts concerning the connexion of facial neuralgia with facial erysipelas are by no means limited to the cases in which the one absolutely terminates in the other. In no less than 22 patients who have come under my care suffering either from well-marked facial neuralgia, or else from severe migraine or from "hysterical" headache, I have discovered the existence of a very strongly marked tendency to facial erysipelas, an attack of which would be brought about by the most trivial causes, such as very slight exposure to cold wind, or, on the other hand, such seemingly different influences as fatigue of body, or unusual mental anxiety or depression. I have now under occasional observation five individuals, in good circumstances of life, in all of whom there is a strong tendency, which is clearly inherited, to facial neuralgia, and an equally remarkable tendency to facial erysipelas, though it happens that in no instance of which I am aware has either of these persons suffered from erysipelas *directly* following a neuralgic attack. In another patient, also in good circumstances, and who recently died at a very advanced age, a strong inherited neuralgic tendency first manifested itself by a distinct outbreak of severe pain, which passed into the worst kind of confirmed tic, shortly after the termination of an acute attack of erysipelas.

Another class of complications which I have particularly observed in facial neuralgias is a series of changes in the nutrition of parts supplied by branches of the fifth nerve. In a former lecture I referred to my own case, and I may now recount it as an illustration of several of these changes. In the first place, a year or two after the commencement of my attacks of frontal neuralgia, and at a time when the pains had been very severe, there occurred a painful thickening and tumefaction of the periosteum of the eyebrow, and also the formation of one or two dense white patches on the cornea, in the centre of which small phlyctenular ulcers formed. About the same time, probably, there occurred a great thickening of the fibrous tissue surrounding the upper end of the nasal duct, which formed a very thick and solid stricture. Some years later, when the neuralgic attacks had become on the whole much less frequent, they recurred with great severity during the state of prostration in which I was left by an attack of choleraic diarrhœa. At this time I first noticed that the hair of the eyebrow was whitened opposite the supraorbital notch, and that grey hairs were thickly strewn over the right side of the head for some time after the attack; and this phenomenon has occurred after every severe attack since that time. It only lasts in intensity for a few days, and the colour soon becomes restored nearly to its natural tint without any falling off of the hair. Unilateral herpetic eruptions (a not very uncommon sequel of one form of neuralgia) have not occurred in my own case. Another nutritive phenomenon which has occurred to me during a spontaneous attack of pain, but which was also produced artificially on one occasion by experimentation with narcotics, is an excessive unilateral development of epithelium on the tongue.

All these several changes I have repeatedly observed in neuralgic patients. I have even seen one of them—namely, the occurrence of fibrous stricture of the nasal duct, which might appear an accidental coincidence in my own case—reproduced under exactly similar circumstances in a female patient; and I suspect that closer inquiry into the history of many cases of epiphora depending on this sort of obstruction would reveal the previous existence of neuralgia of the ophthalmic divison of the fifth nerve.

But there are also other and more distant affections of bodily organs which may distinctly be traced to the secondary effects of a morbid condition of the fifth nerve extending itself to nerves which are centrally connected with the trigeminal. The occurrence of vomiting in attacks of migraine which haue a certain intensity and duration, is a typical example of this kind of reflex phenomenon.

Time, however, will not allow me to dwell on the wide field of observation and enquiry which is presented by these more remote affections; and I must now pass on to the consideration of Diagnosis. It is chiefly with reference to their important bearing on this part of our subject that I have dwelt so particularly on the secondary nutritive changes which are apt to supervene in cases of neuralgia of the fifth nerve.

The first question which occurs for consideration is—how to decide that a painful affection originates in the fifth nerve at all?

Ever since the time of Valleix, most authors have disposed of this question summarily by enquiring as to the existence or non-existence of certain *tender points* at particular situations in the course of various branches of the fifth nerve. Supposing it were possible that a patient should be affected with universal and equally violent neuralgia of all the principal branches of the nerve, the situation in which the most important of these painful points would be developed are such as you see marked on this diagram: 1. The parietal point. 2. The supraorbital. 3. The trochlear. 4. The palpebral. 5. The ocular. 6. The nasal. 7. The infraorbital. 8. The malar. 9. The superior labial. 10. The mental. 11. The auriculo-temporal. [The lecturer gave many details as to the size and form of each of these tender patches.] Pressure on any of these points, even in the intervals of the neuralgic attacks, causes an exquisitely acute pain to dart along to the terminal branches which lie external (peripheral) to the tender spot.

Valleix insisted that no painful affection could be truly styled neuralgic unless one or more of these tender points could be discovered; and he denied altogether the statement which up to his time had commonly been made, that pressure over a neuralgic nerve does not increase the severity of the pain. Other observers, seeing very plainly that many cases of neuralgia clearly do exist in which pressure over the points which I have enumerated does not cause or increase pain at all, have recently proposed to make two classes of neuralgiæ, between which the existence of non-existence of the painful points should form the line of separation.

I feel some confidence in stating my opinion that neither of these views is strictly correct. In all the severest cases of neuralgia which I have witnessed from the beginning, equally with those of a milder type, there was not, in the first instance, any tenderness on pressure over any point in the course of the affected nerve. But in every case in which the pain assumes a certain intensity, and lasts long enough, there occurs more or less tenderness, and sometimes decided swelling and hypertrophy of the fibrous tissues with which it comes into the closest relations; and this, as a matter of anatomical distribution, is precisely what occurs at those points which Valleix so carefully indicated. It would be easy to show that these are situations at which the nerves divide into branches, which pass through the deeper layers of fibrous fasciæ to become immediately subcutaneous. I believe, then, that it is the subacute inflammation, or at any rate hypertrophy, of these tissues which causes the formation of the tender points. This I am sure of, that in one case I have seen attacks of common migraine which were of longer duration than usual followed not merely by general tenderness of the forehead and scalp, but even by the temporary formation of an extremely tender supraorbital point, indistinguishable from that of the most undoubted neuralgia; and, in fact, the disease from that time assumed the latter type. And the cases in which the hyperæsthesia is most excessively developed at special points are precisely those inveterate examples of tic douloureux which occur in the decline of life: the longer these cases last, the more exquisite does the tenderness become. Moreover, it has been observed in the dissection of cases of inveterate neuralgia that the hypertrophy of fibrous tissues, and even of bone, was principally developed around the foramina of emergence of the principal trunks of the nerve; and this agrees in a remarkable manner with the result of the expriments by Schiff and Mantegazza, to which I referred in my first lecture.

I may notice here that Dr. Handfield Jones, who has done so much for the investigation of nervous diseases, so far favours the above interpretation of clinical facts, that he speaks (in his valuable work on "Functional Nervous Disorders," p. 292) of some cases observed by Watson and Brodie (in which *the parts affected with pain at length became hot, swollen, and tender*) as marking a "transition" from a paretic state of the cerebro-spinal nerves, merely, to a paretic condition both of these and of the adjacent vaso-motor nerves.

The test of Valleix therefore, although highly useful in its way, appears to be misleading when used in the sense in which he employs it. Its true use would rather seem to be as a prognostic of the severity and probable duration of the complaint, for there is no doubt that the long-continued existence of two or three or more exquisitely tender points is itself a very bad omen; and on the other hand, the rapid subsidence of this local tenderness is some evidence that the affection is mild, or at any rate is not far advanced.

If these and the other arguments which I have previously used be just, there is then no need to draw a line of radical separation by refusing to consider the milder affections, which we call sick headache, or hysterical headache, or headache of debility, as nerve pains, though it may be well to keep the conventional use of the word "neuralgia" for the severer forms of nerve pain. And on the other hand, in the instances (which I believe to be far more rare than many suppose) in which irritation transmitted from a distant organ is the exciting cause of trigeminal nerve pain, the fact is not to be lost sight of that in all probability the antecedent condition of the nerve itself is the most important factor in the product, which is pain. And as we are on the subject of diagnosis, it may be well to observe that nothing can be more objectionable than that kind of logic which, for instance, when a course of medication with some drug already ticketed "emmenagogue" has simultaneously established menstruation and relieved a neuralgic pain, would therefore decide that the neuralgia had depended on "uterine irritation."

With regard to the diagnosis between the various forms of trifacial nerve pain which are the subject of these lectures, there will generally be little difficulty if the characteristic features which have been already described are relied on as the main guide.

And, finally, with regard to the diagnosis of the situation of the peccant point in the nerve in those neuralgiæ which presumably arise from an organic change: the result of the physiological considerations which I laid before you at our first meeting will convince you that the apparent situation of the pain will in reality tell you little. The physiological law by which sensations that originate even in the most central origins are nevertheless subjectively referred to the peripheral distribution of nerves makes it futile to draw inferences from the sensations themselves. Far more knowledge will, I believe, be gained by a careful study of those complications which I have endeavoured to show you are not merely accidental attendants, but essential portions, of severe painful affections of the fifth nerve. The wider the area over which the secondary phenomena are scattered the greater the probability that the mischief is central, and has thus the greatest number of radiating channels for its secondary influence. And it is this principle which forces us, as I think, to place such affections as migraine and clavus in the same pathological group with the severest tic douloureux, and to separate them utterly from the headaches felt under circumstances of fatigue or "biliousness" by non-neuralgic subjects. Contrast the dull diffused pain (bilateral, and felt nearly always most strongly at the back of the head) which is the simple result of indigestion, with the quick darts of agony, limited strictly to one nerve, and very generally to one division of it, which characterise a true migraine, or "sick headache." And observe, in the latter affection, how, as the severity of the pain increases, two secondary phenomena are developed at the periphery of two nerves which are widely separated except at their one point of connexion in the medulla—viz., the secretory fibres of the fifth, and the vagus. The simultaneous disturbance of these two sets of nerve-fibres is plainly indicated by the occurrence of *intense lachrymation* on the one hand, and of *vomiting* on the other. But there is more to come on this subject of diagnosis which may be more conveniently taken under the head of Etiology and Pathology, because it is essentially mixed up with questions as to the hereditary transmission and transformation of disease.—*Lancet, Aug.* 25, 1866, *p.* 199.

14.—APHASIA ASSOCIATED WITH RIGHT HEMIPLEGIA.

By Dr. Edward Long Fox, Physician to the Bristol Royal Infirmary.

The following cases may add to the collected experience on loss of speech.

Mary M., aged 13, was in her usual health at the beginning of May. She was sent by her mother to a shop, and on her way fell down insensible. She was found to be hemiplegic on the right side, and to have lost all articulate speech.

On admission into the Bristol Royal Infirmary, June 5th, 1866, the right hemiplegia was almost complete. She could not lift the right arm at all, nor flex the fingers; and she could scarcely stand. She could not walk at all. Slight facial paralysis of right side, No paralysis of tongue. Organs of special sense unaffected. Pupils natural. Sensation good everywhere. There was no loss of the general faculty of speech, as she could express herself fairly well by signs, and nodded or shook the head rightly. She evidently understood all that was said to her, and the expression of the face was very bright. But she could not pronounce anything except "ta" and "to," and had lost all power of saying another syllable. She had never suffered from rheumatism; had no valvular disease of the heart; and was a pale, cachectic-looking child.

June 30th. She had so far recovered as to be able to walk about and to move the arm well, but she could not clasp with the fingers. She had learned several words, as "yes," "no," "tea," "bun," &c. She seemed very bright, and read to herself with evident pleasure.

Two or three days after this her mother secretly brought her in some cheese and port wine. She became intoxicated, fell into a succession of convulsive attacks, and sank during the night.

We were only able to examine the head:—Much black discoloration along the fissure of Silvius on the left side. Beneath this region was an old clot, partially discoloured, with creamy brain-tissue all round it, the softening extending slightly into the anterior lobe, but mostly into the middle lobe, and implicating the corpus striatum, except the upper layer of it. This clot was evidently the result of a ruptured left middle cerebral artery. Just at the commencement of the fissure of Silvius the artery was enlarged to the size of a small haricot bean, and this enlarged portion had given way. No plug was found in any of the vessels. The posterior portion of the third left frontal convolution was discoloured for about one-fifth of an inch in depth, and sensibly softened over a space the size of a small walnut. The immediate cause of death was the rupture of a vessel in the left lateral ventricle, on the upper and outer side of it, just above the corpus striatum. The whole of this ventricle was full of clot, which filled all the cornua, and had found its way to a small extent into the right lateral ventricle. The rest of the brain was healthy. Considering the amount of lesion in the corpus striatum, it seems extraordinary that she should have recovered the use of the limbs so well.

I add the following cases of a similar nature:—

Wm. D., aged 47; a great drunkard. Right hemiplegia, with total loss of speech, except that he utters the words "yes," "no," "pooh," "Lord-a-mercy." These he uses indifferently to express everything; and when he is in a passion he will ejaculate "Lord-a-mercy," evidently with the idea that he is using an oath. He can to some extent express himself by signs; as when he can escape from his wife's surveillance he is able to get spirits from a public-house, and pay for them without any assistance. He can write his own name, and understand all that goes on. He has been quite three years in this state; and although the hemiplegia is much diminished, the loss of speech is the same.

John S., aged 35. At ten years old he had right hemiplegia, loss of speech for three months. For six months afterwards he had to learn to speak like a little child. He has now contractions of the right wrist.

Although both these cases are still living, it is probable that they have suffered from a similar lesion to that which caused the symptoms in the first patient, especially when we view the symptoms in connexion with the case quoted by S. Van der Kolk (New Syd. Soc., vol iv, p. 165, note). "In consequence of a wound, a long splinter from the os frontis above the left eye compressed the anterior part of the left hemisphere, subsequently causing loss of speech; which faculty was completely regained after the removal of the fragment by the trepan."

Some very striking cases, mentioned by Abercrombie and Andral, show at least the connexion between the symptoms of loss of speech and lesion of some portion of the left frontal convolutions.

No one can study Broca's views, nor the very excellent papers which Dr. Hughlings Jackson has written on this subject, without acknowledging that there is considerable evidence in favour of the seat of the faculty of articulate speech being located in the posterior portion of the third left frontal convolution. Dr. Jackson's papers are written with so much scientific accuracy, and with so much candour and fairness, that they are models of the result of medical observation; and in a late paper he modifies his adhesion to Broca's views. But, as far as the subject has been investigated at present, I think it has only been proved that this portion of the brain is one of the seats of this faculty, and not the only seat.

In confirmation of this view I would mention—

1. S. Van der Kolk's case. (New Syd. Soc., vol. xi.) Right hemiplegia. Partial imbecility. She had learned to speak, and made known her wants in broken language. Atrophy of left hemisphere of brain and atrophy of right side of body.

2. Abercrombie on the Brain. (Case 37.) Right side convulsed and then paralysed. No loss of speech. Two defined abscesses, containing six or eight ounces of pus, one behind the other, in anterior part of left hemisphere, near the surface. A small abscess in posterior portion of right hemisphere. One would imagine that these two large abscesses must have implicated more or less all the convolutions of this anterior part of the brain.

3. Andral. (Clin. Med., vol. v., p. 392.) No paralysis. No loss of speech. Softening of base of anterior part of left hemisphere over a space the size of a hen's egg.

4. Andral. (p. 66.) Child, aged two. Permanent contraction of limbs, especially of right forearm. Intellect good. Speech good. The pathological appearances are thus described: "On trouva le lobe antérieur de l'hémisphère gauche comme déprimé par un sillon que remplissait une infiltration séreuse de la piemère. Les circonvolutions étaient refoulées en dedans, rapetissées, durcies et jaunâtres: le septum lucidum n'existait point."

5. Mary Ann C., aged 71. (Case of my own.) Left hemiplegia. Speech not lost, but no words could be understood. She sank in the course of two months. Clot in middle of right hemisphere. Arteries of brain atheromatous. An exostosis. the size of a nut, from inner table of left parietal bone.

6. Quoted by Abercrombie. (Appendix, case 32.) Woman, aged 77. Left hemiplegia. Loss of speech. Large cancerous mass in right hemisphere

7. Quoted by Abercrombie. (Appendix, Case 35.) Boy, aged 13. Left hemiplegia. Loss of speech and of memory. Tubercle, the size of an egg, and five or six smaller masses in the substance of the right hemisphere. Pus between arachnoid and pia mater. Turbid fluid in ventricles.

8. Andral. (p. 316.) Right hemiplegia. Loss of speech. Clot in posterior lobule of left hemisphere. With reference to this case, Andral says: "If, then, a former observation, in which we have also seen loss of speech, seems to confirm the opinion which places this faculty in the anterior lobules of the hemispheres, this observation completely nullifies that opinion."

9. Andral. (p. 322.) Woman aged 55. Right hemiplegia. Loss of voice and of speech. Extravasation in middle of left hemisphere, and softening for the space of two or three lines round it.

10. Perhaps this case, quoted by Abercrombie, (Appendix, Case 23,) should be added. Man, aged 26. Severe and increasing headache. Impaired vision and dilated pupil. Paroxysms of giddiness, with blindness, loss of speech, stiffness of the limbs during the paroxysms; then double vision, violent pain in the neck, with convulsive paroxysms affecting the muscles of the neck, and drawing the head violently backwards. Two months after this he had numbness and spasmodic motions of the superior extremities. After seven or eight months he died suddenly in a fit resembling epilepsy. A hard tumour, two inches long and an inch and a half broad, was firmly attached to the tentorium, and imbedded in the posterior lobe of the left hemisphere. It contained an ounce of greenish pus, and the cerebral substance near it was softened.

11. Andral. (p. 454.) Woman, aged 80. Loss of speech. Intellect clear. No motor paralysis. A softening, the size of a large pea, just outside the posterior extremity of left corpus striatum. A softening of similar size exactly in the middle of right hemisphere.

On our present question Andral made the following remarks more than thirty years ago:—

"Professor Bouillard published some years ago a memoir filled with curious facts, from which he thought himself able to draw the conclusion that the formation of speech has as its instrument the anterior extremity of each hemisphere, inasmuch as he found lesion of that part whenever during life speech had been lost. But mark what our researches have shown us with reference to this question. In thirty-seven cases observed by myself, or by others, in relation to hemorrhages and other lesions, in which the alteration was situated in one or both of the anterior lobules, speech was abolished in twenty-one cases and preserved in sixteen. On the other hand, we have collected fourteen cases in which speech was abolished without any alteration of the anterior lobules. Of these fourteen cases seven were connected with disease of the middle lobes, and seven others with disease of the posterior lobes. Loss of speech is not then the necessary result of lesion of the anterior lobes; and besides, it may take place in cases where anatomy shows us no alteration in these lobes. We will add that M. Lallemand has cited a case in which no other alteration was found than softening of the white substance of the left lobe of the cerebellum; in this case the faculty of speech was completely lost. You will read also in M. Ollivier's work on the Spinal Cord an observation of an individual in whom one of the chief phenomena was the loss of speech, at first incomplete, and then total: in this case the meso-cephale was found softened at its interior surface to an extent equal at least to a filbert."

I have thought that these cases might help in the investigation of a very interesting subject.—*Lancet Aug.* 11, 1866, *p.* 145.

15.—TREATMENT OF DELIRIUM TREMENS BY CAPSICUM.

By Dr. Lyons, Richmond Hospital, Dublin.

[The patient was a man 40 years of age.]

When seen on the 25th March he presented the well-marked phenomena of delirium tremens, and for many days and nights had had no sleep.

Dr. Lyons having, on more than one occasion, previously employed with success the treatment by capsicum, specially recommended to his notice by his friend, Dr. Kinnear, Director of the Melville Royal Naval Hospital, Portsmouth, determined to give it a further trial in this case, in which, for various reasons, it appeared to be indicated, and ordered the patient thirty grains of the powder of capsicum, to be made into a bolus and administered immediately. The dose was taken without any difficulty, notwithstanding that some slight burning sensations were felt in the mouth and throat for a time, and a sense of diffused warmth through the stomach and bowels for a brief period subsequently. In less than one hour after the bolus was taken he fell into a quiet sleep, and some three or four hours subsequently awoke, perfectly calm, conscious, and convalescent. It is much to be regretted that notwithstanding that he was perfectly reasonable, and in all respects quite free from any symptoms whatever of the condition of delirium tremens, a draught, containing 30 minims of gutta nigra was administered about 10 a.m., this medicine having been ordered in the morning, to provide for the possible failure of the capsicum dose; but of the efficacy and satisfactory result of the former no reasonable doubt can remain. No stimulants were employed at any time in this case. Convalescence was rapidly established, and the man left hospital in a day or two, himself and his wife in no little degree surprised at the almost magical effect produced by the dose of so familiar an article as that employed.

The results obtained by Dr. Lyons in the use of this drug fully bear out the experience acquired on a far larger scale of observation in the West Indies and in the Melville Hospital by Dr. Kinnear, Dr. Lawson, and others of his distinguished colleagues in the public service at home at abroad. In the records of the Melville Hospital not less than from seventy to eighty cases are reported to have been successfully treated by the sole use of this drug in single or repeated doses, ranging from one scruple upwards. No gastric disturbance or other unpleasant symptom has been at any time noticed.

As a stimulant of great and immediate efficacy, Dr. Lyons considers that its action may be explained by the direct influence it exerts upon the gastric expansions of the vagi, and so indirectly upon the cerebro-spinal centres. The phenomena of the disease he considers to point to a double condition of stimulated excitation and partial paralysis of distinct and perhaps opposite portions of the nervous system.

For general employment it cannot be doubted that, as pointed out by Dr. Lyons, the use of capsicum offers many advantages over either opium or digitalis. In cases of recurrent delirium tremens associated, as they often are, at a somewhat advanced period of life, with fatty degeneration of the heart, both the latter drugs are very distinctly contraindicated, and their use has not infrequently been attended with results far from satisfactory, even when free from fatal result, which has not always been the case.—*Med. Press and Circular, April* 18, 1866, *p.* 395.

16.—CASES OF SCIATICA, DEPENDENT UPON APPARENT OR ASSUMED STRUCTURAL LESIONS.

By Dr. William Leishman.

The following cases were presented in the course of a clinical lecture in illustration of the etiology of sciatica, in that class of case where the ordinary symptoms are the result of structural changes, either in the tissues in juxta-position with the nerve, or in distant organs; the disease being in these instances merely symptomatic of other affections. It would be obviously erroneous to rank such as ordinary cases of sciatica, and still more so were we, in view of the whole facts, to treat the mere symptom as if it were the disease. The cases are in themselves instructive, as showing how treatment is so frequently inefficacious. In some, the symptom being at first in no way distinguishable from those of ordinary sciatica, the usual treatment was adopted, but without any satisfactory result beyond a mere temporary alleviation of the pain. Indeed, it may be said that in all the cases, their true nature was only recognised after prolonged and careful investigation. The cases are still under treatment, but as far as this has gone, it must be confessed that it has been, on the whole, unsatisfactory.

Case 1.—*Sciatica, associated with Congestion of the Uterus, Leucorrhœa; symptoms relieved by leeches to os uteri.*

Ann M., aged 38. Admitted June 29th. Patient, who has been married for some time and has had three children, states that her present illness commenced about seven weeks ago. Although never a very strong woman, being subject to what she calls "fits of debility," when her strength appeared to leave her altogether, she has always been tolerably healthy up to this present attack. Her labours have all been natural, the children healthy, and menstruation most regular. Seven weeks ago she first began to experience pain in the lower part of the back, extending down the leg, sometimes as far as the toes, the right leg being the only one affected. This pain has never left her and she has been getting weaker and more helpless, walking now with a decided limp. About nine weeks ago, a fortnight before this pain commenced, she noticed a white discharge from the vagina which has continued, more or less, up to the present time, but is not so bad now as it has been. She refers the pain to several positions, the spot where it is worst and most constant being immediately over the sacrum. It is also felt in both buttocks, most severely in the right, extending down the right thigh. It is to some extent constant, but has paroxysms of severity, during which it occasionally passes across the back into the groins in front, but this is rare. When sitting up she has a feeling of weakness up the whole spine. General health is good, although her strength has been failing her of late. Bowels have a tendency to be costive unless relieved by medicine.

July 8th. Fly blister to be applied over sacrum. The white discharge from the vagina has, at this date, ceased.

July 10th. On examination with the speculum the os is seen to be slightly open, with a white, glairy discharge exuding from it. Lips and cervix enlarged and somewhat congested, but without any ulceration or abrasion of the mucous membrane. She experiences considerable pain when pressure is exercised on the os, or in its neighbourhood. Leeches to be applied to os

July 12th. Two leeches were yesterday applied to the os and a considerable quantity of blood withdrawn; this was accompanied by an almost immediate relief from pain both in the back and thigh. This morning the improvement is maintained and she says that she is very much better than she has been at any time since admission, although there is still some pain in the right thigh.

This case requires no comment. The opinion that the pain in the course of the right sciatic nerve was dependent on uterine irritation or congestion was strengthened on examination by the speculum, and was confirmed by the result of local depletion from the important organ in question. A repetition of the bleeding was followed by similarly good results; and at the present time, while this report is passing through the press, the patient may be said to be nearly well.

Case 2.—Lumbago and Sciatica connected with, and probably dependent upon some affection of the nervous centres; Paralysis agitans; Abnormal Sensations.

Daniel G., aged 60. Admitted June 29th. Rather more than a month ago patient began to complain of a severe pain across the back, extending down to the right leg as far as the knee. This has continued up to the present time, almost constantly present, but at times almost unbearable. He refers the pain to a line corresponding to the course of the sciatic nerve. Three weeks ago (previous to his admission) he was severely cauterized by Corrigan's iron down the back of the thigh, numerous small, round, suppurating eschars still remaining, and this treatment gave him some relief, at any rate lessening the pain in the thigh, the worst at present being in the right buttock. Patient has also a pretty severe cough, with a copious frothy expectoration. He appears to be in a nervous condition especially when spoken to, and his hands shake much when he attempts to move them. He seems startled, and answers questions in a hurried, confused manner. His general health is tolerable, appetite unimpaired, but he complains of weakness, and a constant sensation of chilliness even when in bed. Bowels habitually costive, chest and heart sounds normal, with the exception of some slight bronchitic râles. Urine contains no albumen. Ordered the acetate of potash, and recently the nitrate of bismuth for some irritability of the stomach.

Case 3.—Sciatica, treated by Corrigan's iron, acetate of potash, quinine, and subcutaneous injection, in succession, but without benefit. Discovery and development of a large abscess.

Alexander Maxwell, aged 26. Admitted May 22nd. Patient admitted complaining of almost constant pain in the left leg, extending from the buttock down the back of the thigh to knee.

He states that this first began to trouble him about six months ago, it has been much worse lately, and he is now scarcely able to walk, much less to work, the pain being aggravated on the slightest exertion. On examination, no swelling of any kind can be detected, with the exception of some enlargement of two or three lymphatic glands on the inside of the thigh. Corrigan's iron to be used.

May 25th. ℞. Potass. acetat. ℥ss ; aquæ ℥xxiv. s. Sig. A wine-glassful three times a-day.

June 15th. No improvement. Patient complains somewhat of loss of appetite. A strong solution of the acetate of morphia was yesterday injected subcutaneously, but even this was only followed by a very slight temporary alleviation of the pain.

℞. Quiniæ sulphat. ʒi ; acid. sulph. dil. ʒiss ; aquæ ℥viii. s. Sig. A tablespoonful three times a-day.

June 28th. A large, deep-seated abscess is forming in patient's left gluteal region. He has had several severe rigors, and has lost flesh considerably of late. Pulse rapid and compressible, appetite bad. To have 8 oz. of port wine daily.

30th. Hectic symptoms more marked. The fluctuation is now more distinct, and the abscess appears to come nearer the surface day by day.

July 3rd. This morning the abscess was opened by an incision in the middle of the buttock, a large quantity of pus escaping.

11th. Patient still in a very weak state ; complains of very profuse perspirations. The discharge is still abundant, but sinuses seem to be forming down the thigh from which the matter is only dislodged by pressure.

Case 4.—Case of Sciatica, dating from an injury. Unsuccessful treatment. Probable local lesion.

Ward 1. Bartholomew C., miner, aged 18, admitted May 23rd. Five months before admission patient was bruised at the lower part of his back by a hutch coming against him. He was confined to bed for a short time, but was soon at work again. About six weeks after he received the injury, he began to feel pain in the right buttock, going down the back of the thigh as far as the knee, and sometimes extending as far as the ankle. Immediately after the accident the right leg lost feeling and power, to a slight extent, and even at the present time patient occasionally has a feeling of weakness and want of power in it. The pain in the buttock is constantly present, and is worse when patient is in a sitting posture. The pain is not aggravated by moving the limb, nor is there any shortening or swelling. He sometimes experiences pain over the lumbar vertebræ, especially on stooping, exactly on the spot where the bruising from the hutch was most severe. There is no swelling or undue vertebral prominence in this situation. General health good.

June 16th. To have five drops of Fowler's solution three times a-day.

June 30th. Complains of some tingling of the eyelids. Tongue covered with a silvery white fur. Fowler's solution to be continued with caution.

July 3rd. The constitutional effects of the arsenic being now manifest, this to be discontinued. The subcutaneous injection of morphia, and acupuncture, have both been tried without success. Blister to be applied to the painful part

July 12th. Patient not in the slightest degree improved. His general health is beginning to fail.

Case 5.—General Neuralgia with Sciatica, successfully treated by the Bromide of Potassium. Recurrence of symptoms. Disease of the spinal column.

Ward 1. Francis K., aged 26. Admitted May 8th, 1866. Patient was in the hospital rather more than a year ago, under treatment for the same symptoms that he is at present suffering from, namely, pain in the back, shoulders, and limbs. He remained under treatment for some time, underwent various changes of treatment, and ultimately improved rapidly under the bromide of potassium, which was given when all else had failed. He has had no return of the complaint until six weeks ago, when the pain re-commenced in the back and shoulders, disappearing when at rest, but coming on when he is walking or making any exertion. It is getting worse of late, and he can scarcely move with it at present. He refers the principal pain to the back, extending from the dorsal vertebræ downwards, and to the left side, but it varies considerably as to position, and occasionally travels down the legs or affects both shoulders very severely. General health good. There is no swelling of any of the joints. Chest and heart sounds normal.

℞. Potass. bromidi, ʒi. ; aquæ, ℥viii. s. Sig. A table-spoonful three times a-day.

July 11th. Patient, though at first much benefitted by the bromide of potassium, has of late been getting gradually worse, the pain, especially in back, being more severe than on admission, and patient is in a state of great weakness from his protracted illness. He cannot walk now at all, and any movement in bed is attended with great pain. On carefully examining the spinal column, the spinous process of one of the dorsal vertebræ (the third) is found to be abnormally prominent, and there is marked tenderness on pressure at this point. General health of late has been failing rapidly ; he takes but little food and has lost flesh and strength.—*Glasgow Med. Jour., Aug.* 1866, *p.* 155.

DISEASES OF THE ORGANS OF RESPIRATION.

17.—ON TUBERCULOSIS.

(An Abstract from Virchow's Krankhaften Geschwülste.)

By Dr. **Francis Delafield**, New York.

One of the greatest living pathologists has definitely formulated his views concerning that most difficult subject—tuberculosis. No English translation of his work has yet appeared. This short sketch of his treatise may be of interest.

The lymphatic glands consist of cells, the so-called lymph cells, contained in a fine reticulum of connective tissue, and arranged in follicles divided by fibrous sheaths. These follicles may form large masses, as in the thymus, the tonsils, and in Peyer's patches; or they exist singly, as in the solitary intestinal glands, and the malpighian bodies of the spleen. The essential element is, in all cases, the cells.

There are two groups of tumours analogous in structure to these lymphatic glands. First, hyperplastic growths of already existing glands; second, heteroplastic growths of the elements of glands, where none such normally exist. To the second of these groups belong tubercles. There are two words which have been so loosely used in connection with tubercles, namely, scrofula and struma, that it is necessary, at the outset, to define them.

Scrofula is the literal Latin translation of the Greek chœras, which is found in Hippocrates. Both expressions signify a young pig (scrofa, χοιρος). The older writers derive the name from the fact that the swellings are as numerous as a sow's young; or that swine suffer from this disease; or that swine have necks containing many glands; or that an affected neck assumes the shape of a swine's. The Latin word, however, was little used by the ancients, and the expression "scrofula" has only been generally used since the time of Cullen and Hufeland.

The word struma is found in translations of Greek authors, and in Celsus, as a parallel expression to scrofula, often with exactly the same meaning. This original use of the two words as synonyms has been reproduced by modern English writers, who express by "strumous" what continental writers call "scrofulous," or "tuberculous." French writers use the word struma very little. German authors, on the other hand, express by struma, tumours connected with the lymphatic glands. This use of the words will be here retained. Scrofula, however, is here used to express not a mere swelling of the lymphatic glands, but a peculiar condition of the constitution, which causes the lymphatic glands to be unusually vulnerable to any irritating cause, and indisposed to healthy reparative action.

This condition can be explained in part by an unusually rich development of the lymphatic organization, in part by a weakness of particular parts or regions. This weakness is caused by a certain imperfection in the organization of the glands. Such a constitution may be hereditary, or may be produced by insufficient and bad nourishment, foul air, &c.

The word tubercle had originally nothing to do with any special process, but merely expressed the shape of some particular local growth, or was even used as a synonym for processes of the bones. So it was applied to tumours of the most diverse natures, syphilitic, cancerous, bony, and fibrous, as a simple descriptive term. The word first began to be used in its modern sense at the end of the last and the beginning of the present century, at the time when more accurate anatomical investigations of lung diseases, especially by Baillie and Bayle, were undertaken.

Careful *post-mortem* examinations of morbid lungs revealed a variety of conditions, which were called by various names: tubercula, struma, scirrhoma, steatomata. Two forms of phthisis were distinguished, one resulting from pneumonia and catarrh, the other from tubercles. The tubercles were considered to be diseased glands. Attention was called to the many points of resemblance between tubercles of the lungs and scrofulous glands, and hence was involved the doctrine of the identity of scrofula and tuberculosis, a doctrine held by Von Swieton, Morgagni, Cullen, Portal, and Hufeland.

The exclusive examinations of the lungs, however, and the regarding them as a standard of tuberculosis, led to confusion. Launec, especially, investigating as a specialist, and considering phthisis as a unity, confounded together a number of totally different conditions, and his great authority has influenced nearly all subsequent investigations. His followers held the cheesy material as the diagnostic sign of tubercles. Even those who, like Lebert, declared against the identity of scrofula and tubercles, considered the cheesy condition of the glands as a diagnostic sign of a tuberculous process. This cheesy material has been the source of numberless errors. It must be borne in mind that it is no specific material, but is simply dead tissue, and may be the last stage of various morbid processes. Any reasonings which regard this dead material as the essential part of tubercles must end in error. Thus, Broussais and Cruveilhier, considered tubercles as the result of an inflammatory process, and originated the doctrine of tuberculous inflammation. It is absolutely necessary to hold fast the non-identity of the original processes, and to overlook the identity of the metamorphosis which the tissues can undergo. Then it becomes possible to make the essential distinction that tuberculosis, in opposition to scrofula, is the production of heteroplastic lymphoid new growths in regions where they do not belong.

The true tubercle has no essential connection with inflammation. Whether its growth is, or is not, attended by inflammatory process, its character remains the same. It is, however, undoubtedly of an irritative nature, and it is even right to speak of a tuberculous inflammation.

Though tubercles are to be considered to be as distinct from scrofula, it is necessary to admit their near relationship. Tuberculosis may even be regarded as an heteroplastic scrofula, for the frequent occurrence of both conditions in the same person is otherwise difficult to explain.

There have been various views in regard to the relation between tubercles and the products of inflammation. First, that tubercles are the irritating cause which produce the inflammation. Second, that both tubercles and inflammatory products are formed from a simultaneous exudation. Third, that tubercles are produced from the inflammatory products. The first and last of these views are founded on fact, and can be proved by observation. The second view, that of a tuberculous exudation, was originated by Magendie, and supported by Rokitansky and the Vienna school. They held that the specific material was exuded from a morbid blood, and cited the existence of the well-known cheesy material in the alveoli of the lungs as proof. The result of their reasoning and mode of investigation was that the real tubercles of the lung were overlooked. And under the name of gray granulations, in the lung and arachnoid membrane, they have been described by Robin as something new and distinct.

It is in the lungs that the cheesy material has caused the greatest confusion of ideas. After a chronic pneumonia or bronchitis, the alveoli and small bronchi are left filled with the products of inflammation. These thicken, degenerate, and become cheesy; there results what has, since Lannec, been called "tubercular infiltration," but is really a cheesy hepatization. This cheesy material may be found in miliary form, in circumscribed deposits, or involving entire lobes. True tubercles of the lungs arise always in the walls of the air passages, and are not secreted in their cavities.

To avoid confusion, it must be remembered that tubercles exist in various stages of growth and decay, and vary somewhat in different organs. A description, therefore, true of one stage, may be quite false of the others.

The true tubercle is organized, if not vascular; that is, it is composed of living cells. It arises from connective tissue, bone, fat, or marrow. It is, therefore, best studied in those parts which are composed of the simplest tissues, such as serous and false membranes; next, in glands, with a well-defined stroma, as the liver and kidney; with the greatest difficulty in organs, like the lung and brain, of a complex structure.

The young growth looks at first like fresh granulation tissue; it contains very soft, fragile cells and nuclei. These cells are the true tubercle corpuscle, which is not a mere nucleus nor a solid body. They resemble essentially the lymphatic gland cells, are round, and vary in size from a little smaller to threefold that of a white blood corpuscle. The cell body is colourless, transparent, a little granular, and easily broken by pressure or the addition of water and reagents. The nuclei are small, homogeneous, shining, containing nucleoli, and number from one to twelve in a cell. Between these cells is a small, net-like arrangement of connective tissues, fibres, and sometimes vessels. The latter are usually not new, but belong to the old vessels of the part.

Lebert's tubercle corpuscle is no original element, but a production formed from cheesy metamorphosis. It can be found not only in dead tubercles, but in pus, scrofulous glands, cheesy hepatization, and carcinoma, after they have undergone the cheesy transformation. It has, therefore, no diagnostic worth whatever.

The young tubercle is a true neoplasm—arises not from an exudation, but from proliferation of existing tissues, or from newly formed connective tissue.

The cellular arrangement of tubercles is repeated in all parts where they reach their acme. But in many regions the acme is never reached, especially in firm, fibrous tissues, and newly formed connective tissues. Here a large part of the tubercular tumour consists of thick connective tissue, whose cells are numerous and contain several nuclei, while only in the centre is a riper growth found. When such a tumour becomes older nothing will be found but a fatty, granular centre and a shell of connective tissue—no cells.

After the first development of tubercles their regular course is to the cheesy transformation, but fatty degeneration, with or without resolution, may also take place. The cheesy transformation begins at the oldest part of the deposit, generally the centre. After the cheesy stage comes that of softening, which also first attacks the oldest portion. In tubercles growing on surfaces, however, the oldest portion is the middle of the surface, and not that of the entire growth. Those who suppose softening begins at the periphery have only observed conglomerate masses, or non-tubercular cheesy deposits. The softening is not the result of the tubercular mass causing inflammation and suppuration of the surrounding tissues. It is a purely chemical process, unconnected with suppuration. The debris of tissue, which form the cheesy mass, separate into smaller and smaller elements, and may even change to a fluid form.

If the softened tubercles are near the surface, as in mucous membranes, there follows ulceration. This takes place through the simple separation of the softened mass, without any suppuration. But as the softening is usually only partial, the bottom and walls of the ulcer are still formed of cheesy material, which gradually also softens and separates, until there is left an ulcer no longer tuberculous, though caused by tubercles. Not until it has thus become a simple ulcer does it secrete pus. These ulcers can be best studied in the bladder. After the separation of the tuberculous matter the ulcer may cicatrize, but this is seldom the case. More often new growths form around and under the ulcer, and the morbid process is constantly beginning afresh. The so-called infiltration is formed when a number of deposits are situated near each other. Through their confluence is formed a continuous, homogeneous, cheesy conglomerate. In mucous and serous membranes, through such a confluence of miliary tubercles results a thick, yellowish white, dry layer, which covers the entire surface, like a diphtheritic membrane If this takes place in the walls of a tube like the bronchi or ureters, it may even obliterate their canals; and if the mass afterwards softens, it will appear like an exudation in the cavity of the tubes.

Large tubercular masses are best studied in the brain and spinal cord. There it can be seen that the mass is formed of lamellæ, and that the growth takes place by the apposition of new gray tubercles, and not of cheesy material.

In the lymphatic glands, there exists a tubercular growth arising from their connective tissue. The glands usually inflame and hypertrophy at the same time. The growth begins as small, grayish spots, in greater or less number, but does not always affect the entire gland. The gland tissue proper becomes soft, reddish gray, and succulent. The gray spots become larger, firmer, harder—and, finally, cheesy. Afterwards the mass may soften. Tuberculosis of the glands is nearly always secondary to that of neighbouring organs.

The spleen is one of the favourite seats of tuberculosis. On the other hand the tonsils, the salivary glands, the pancreas, the muscular system, except the heart, the thyroid gland, the mammary glands, and the ovaries, show an unaccountable indisposition to take on this process.

The testicles are strongly predisposed to tubercle. The existence of syphilitic growths and of chronic inflammatory processes renders their diagnosis obscure. The anatomical diagnosis of the inflammatory process is not difficult. The gummy tumours are to be distinguished by their situation in the body of the testicle near the tunica albuginea, while tubercles usually begin in the epididymis. The tubercles always rise from the connective tissue, and never from the epithelium,

In bones, tuberculosis usually arises from the marrow, especially in the spongy bones. The vertebræ and the ends of the long bones are its favourite seat. The process usually takes the form of an osteo-myelitis tuberculosa, though in young children a simple formation of tubercles occurs. The yellow marrow first becomes red, then are formed small, grayish granulations, at first scattered, later, grouped together. The surrounding marrow is hyperæmic. Later, these granulations becomes cheesy, run together, and there result opaque, yellow masses, which contain the detritus of the surrounding tissues. These partly cellular, partly dead masses fill the medullary cavities. At the same time the bones thicken. After a certain time the bone tissue itself is affected, and this may take place in two ways. First the bone tissue changes into soft granulation tissue, in which miliary tubercles grow; or, secondly, the bone surrounding the cheesy masses necroses, especially in the spongy bones. There results a form of caries. Around such dead portions of bone arises a secondary inflammation and suppuration; hence are formed abscesses, which seek the surface by fistulous openings.

In Pott's disease of the spine, the cause may be either such a tubercular process, or more often a true inflammation and suppuration of the bone—osteo-myelitis scrofulosa.

If we now consider tuberculosis as a whole, we will notice two characteristics: its heteroplastic formation, and its inclination to multiple eruptions. Both these qualities seem to imply a dyscrasic cause, and the doctrine of a tubercular dyscrasis, or diathesis, has been widely taught and believed. Hence, also, arose the question as to the exclusion and combination of tubercles with other diseases. It may be safely asserted that there is no exclusion of tubercle against other diseases, only against certain organs and tissues. But it never forms part of a mixed tumour. This question loses its interest when tubercle is considered, not as an exudation, but as a new growth.

But now we must ask, whence and how does this growth arise?

It can be definitely stated that connective tissue and its allies are always the matrix. The attempts at determining the cause of the new growth by experiments on animals have proved very unsatisfactory. It is doubtful if true tubercles even exist in them. No one has yet succeeded in forming tubercles by experiment.

There is certainly a local vulnerability and a little immunity of organs. In general, organs normally containing lymphatic elements are those most predisposed to the disease, but there are exceptions which cannot be explained. Also there is a vulnerability and immunity of individuals.

Tubercles are a disease of extra-uterine life; they are hereditary, but not congenital—hereditary not as a disease, but as a disposition. It is probable that not only tubercles, but also syphilis, scrofula, and other diseases of parents may cause a predisposition in their children.

. The tissues are the carriers of this predisposition, and the younger they are so much more easily is their disposition excited. A disposition to tuberculosis indicates always a disposition to inflammation. Childhood and youth are especially prone to the disease. The fact that in the same family one child is attacked by tubercular arachnitis another by tubercular osteomyelitis, a third by tubercular laryngitis, does not prove the existence of a dyscrasis, which breaks out now in one organ, now in another. It rather shows that different exciting causes affect different regions, all having the same predisposition. The prediposition is not only hereditary, but is produced by all causes which debilitate the general system.

Tubercle resembles malignant growths, in that it infects neighbouring tissues. Thus, in mucous membranes and in other organs, the original growth causes the formation of new growth in its neighbourhood. There is also found a secondary tuberculosis of the glands, as in the mesenteric glands after intestinal tuberculosis, and in the bronchial glands after tubercular bronchitis. Metastases in distant organs, also, are produced.

The contagiousness of tubercles or their inoculability has not yet been demonstrated.

It seems probable that tubercles may be at times epidemic. It may be that, as with plants, so with tumours, certain seasons of the year produce an increased growth. These questions require further study.

The indications of treatment are: When possible, extirpate the tuberculous mass early, as in the testicle, the glands, the bones, and joints. When this is not possible, we must, first, fight against the predisposition by every means which will improve the general health; and, secondly, carefully avoid all irritating causes, for a slight catarrh or inflammation of no moment in a healthy constitution, in one disposed to tubercles brings a new growth in its train.—*Medical Mirror, July* 1866, *p.* 392.

18.—ON THE HYGIENIC TREATMENT OF PULMONARY CONSUMPTION.

By Dr. J. Henry Bennet.

[There can be no doubt that the deposit or exudation of tubercle in the tissues, which in the lungs constitutes phthisis, is the result of defective nutrition, consequent on defective vitality inherited or acquired. Consequently, the rules for treatment become to some extent self-evident; they must be found principally in the strict observance of the laws of hygiene.]

The laws of hygiene may be considered to embody the conditions, bodily, social, and mental, which are the most favourable to the healthy development of the human economy, the most conducive to its well-being. These conditions have only been clearly elucidated by modern research, and are daily ignored and infringed by the immense majority of the human race—with comparative impunity by the strong, the vigorously constituted, but not so by the weak, by those who are born with defective vitality, or are living in unhygienic conditions. In both cases existences which, although weak, might have reached the ordinary term of human life under favourable conditions, are prematurely brought to a close.

Bodily hygiene includes principally good and abundant food, pure air, a clean skin, and exercise. Theoretically, the injunction to scrupulously attend to these points seems so rational in a disease of debility that it appears scarcely necessary to lay stress upon them ; but practically it is not so. A large proportion of the medical profession, instead of looking upon the progressive deposit of tubercle in the lung, with its gradual softening, upon the hemorrhages, and the bronchial and laryngeal affections which it occasions or which precede it, as mere local symptoms of a general diathesis, have their attention arrested by the local condition. They exaggerate its inflammatory nature, and dare not apply to their patients the ordinary rules of hygiene ; they dare not give wine and plenty of animal food ; they dare not give fresh cool air day and night; and they dare not keep the skin clean and cool by cold or tepid sponging. Yet this timidity is a fatal mistake, for these are the principal means by which nutrition is to be improved and restored to a normal condition, and consequently by which the disease is to be arrested and cured.

The food taken by consumptive patients should be of the most nourishing kind—meat, fish, fowl, eggs, milk, bread,—well cooked, and abundant in quantity. Indeed, the quantity of food taken should merely be limited by their digestive powers. In my opinion, the principal value of medical treatment in phthisis is in the restoration of digestive tone when impaired or absent. If patients can be brought to eat, to digest, and to assimilate, they have a chance of recovery. If they cannot, their chance is indeed slight.

The medical attendant, however, must never forget the important fact which I developed at length in my work on "Nutrition in Health and Disease," published in 1856—viz., that there are two great types of digestive power, the quick and the slow. In many persons, in most indeed, the digestive process is rapid. Such individuals require food often—three, four, or more times in the twenty-four hours, and in that period they can take and digest animal food two or three times. If they have not frequent meals they feel faint and ill. The other class digest more slowly, more laboriously. They can only take food with advantage and comfort to themselves twice, or at the most three times, in the twenty-four hours, and only one meal must be a meat meal. Such individuals become dyspeptic if they try to assume the habits of those who require more frequent meals. The real remedy for their dyspepsia is not physic, but the adoption of a dietary more suited to their constitution. These peculiarities remain in disease, and must be attended to if the patient is to do well. There is no rule but the patient's own individual constitution. It is worthy of remark that the people who make two or three meals only, evidently get more out of their food than those with quicker digestion, and consequently can do well with less food.

A moderate amount of wine, as a tonic and gentle stimulant to digestion, I consider beneficial—say six or eight ounces; that is, three or four glasses of claret, burgundy, hock, or two of sherry, taken with meals, and diluted with water; or a glass or two of bitter beer if the stomach can bear it. Of late years, in America, whisky has been much lauded as a cure for consumption, on what rational ground I am really at a loss to conceive. I have seen a certain number of cases in which it had been long taken, but I cannot say with benefit. The daily ingestion of large quantities of nerve-stimulating spirit certainly does not come under my notion of hygienic treatment. Carbonaceous food can be given to all but any extent in a more natural and less pernicious form.

It seems also, at first sight, as superfluous to state that in a disease of debility like pulmonary consumption patients should breathe pure air as that they should live on nourishing food; but it is not so. Theoretically the value of pure air—of atmospheric food—is universally accepted by the medical profession; practically it is all but universally neglected. The physiology of respiration—a modern discovery—has yet to be applied, not only in every-day life, but even in the treatment of disease. Most medical men as well as their patients ignore the all-important fact that the demands of respiration are so great that one or two human beings soon use up and contaminate the air contained in a good-sized room. Such being undeniably the case, unless it is renewed artificially, or by an open window or door; in other words, unless the air in an inhabited room is constantly undergoing change, impure air is ever breathed—air calculated to produce disease in the healthy, and to increase it in the sick. So universal is the neglect of this fundamental law of health, that the healthy persons who do not sleep in rooms with the windows, doors, and register stoves shut, and who do not thereby poison their blood all night with their own excreta, are as yet quite the exception. In ill health, and especially in diseases of the respiratory organs, the dictates of science and of common sense are still more grossly outraged. At a time when, perhaps, the principal food the economy can take is pure air; when the diseased lungs, partly inefficient, require the purest and best air-food that can be afforded them, the doors and windows are generally kept shut on pretence of chills, cold air, and draughts, a due supply of respirable air being thus refused to the unfortunate patient.

Attention to the functions of the skin is, I consider, next in importance to attention to food and air—that is, to digestion and to respiratory nutrition. The skin has very important eliminatory functions to perform. It is by excretion through its pores that the economy partly throws off the effete or used-up carbonaceous and nitrogenous elements of the system. This is illustrated by the strong odour of the cutaneous secretion when not washed off. Moreover, the skin and the lungs seem to partly replace each other in this work of excretory purification. In warm summer weather the skin and liver act freely, and the lungs and kidneys are comparatively at rest. In the cold damp weather of winter the pores of the skin are closed, and it rests, the lungs and kidneys taking up the excretory process. Thence probably the feverish colds of damp cold weather. The blood is poisoned with the elements that the closed pores of the skin should have eliminated, which occasions the fever; whilst the lungs often succumb to the increased duties they have to perform, and inflammatory affections supervene. Whatever the explanation, the fact is certain, and it is now well established that the best mode of preserving the respiratory organs from winter colds is to keep the pores of the skin open by the use of cold or tepid water, combined with friction; or, in other words, to keep the cutaneous excretions up to their normal standard.

Acting in accordance with this view, I make all my consumptive patients, whatever their condition, if they have the strength, use a sponge-bath at the temperature of from 64° to 68° daily, and with the greatest possible benefit. I neither have to contend with hemorrhage or chills, nor with aggravation of the cough, but quite the contrary. The cold sponge-bath produces in nearly every instance a feeling of indescribable comfort, and lowers the pulse. The contact of the cold water may accelerate the expectoration of the muco-pus collected during the night in the bronchial tubes, but that never alarms when it is explained that such a result is naturally to be expected. I myself derived the greatest possible comfort and benefit from cold sponging in summer in the open air on the banks of a Scotch loch, the waters of which were at 60°; and that when I was very ill, pulse 100, and skin hot and feverish. This gave me a confidence I have never lost, and of which I have never had reason to repent.

The question of exercise is an important one, and one that requires discussion and elucidation. I would say at once that, from personal experience and observation, I believe it is a great mistake for consumptive patients to take much active exercise. Every winter I see some such patients walk themselves to death. They have been told by their medical attendants at home to take exercise, and they do so, thinking that what gave them an appetite, and did them good when well, will do so now they are ill; but they merely walk themselves into their graves. The disease from which they are suffering is one of debility. The strength of former days has gone out of the youth and of the man, although perhaps he knows it not. Or the strength he has is fictitious, unreal strength, the result of a febrile condition, of a state of morbid nervous excitement. So he walks up hill and down dale, loses his appetite, cannot eat, becomes "bilious," is dosed for liver, and the disease progresses rapidly. Every winter, towards January or February, some invalids consult me who have up to that time taken their case in their own hands, and have thus walked from breakfast to dinner, with the healthy, in order to gain strength. But they have lost it instead—have become paler and thinner; and when I see them, I find that they have lost ground, that the disease has gained upon them since they arrived in the autumn, and that they are decidedly worse—all from over-exercise.

The sound rule for a consumptive patient is to take passive exercise, not active; to ride in an open carriage; to be rowed in a boat; to sit and lie hours in the open air; to live with windows open, but never to incur great muscular exertion. The amount of vital power in such cases is small. If it is too freely expended in exercise, there is not enough left for normal digestion; food is imperfectly assimilated, nutrition is defective, and the disease progresses.

A singular, but explicable fact is, that during the existence of active disease, when tubercles are forming and softening, very often no lassitude is felt on exertion. But when the disease is arrested, and a curative process has been set up, extreme debility and lassitude may be experienced and complained of, lasting for months, or even years. I felt this lassitude for five years. The explanation is simple. As I have already stated, in active disease there may be a false, feverish strength, like that of the delirious patient whom it takes half a dozen men to hold. In the curative stage, the false strength is gone; the real condition of the patient comes to light, as it does with the delirious patient when the delirium is gone, and he can scarcely lift his hand from the bed.

The social and mental hygienic condition favourable to the treatment of consumption may be summed up in a few words—Rest, repose, the absence of the ordinary duties, cares, harass, and worries, of life. To obtain these is difficult in the social medium in which the disease has appeared. Therefore, the duties and obligations of life should be surrendered for a time, if possible; modified, diminished, if not. Those, however, have the best chance of arresting the progress of disease who can escape from the social medium in which it appeared. To do this it is always necessary to make great sacrifices—sacrifices which many cannot make. But those who can must remember that the struggle is one not merely for a higher or lower stage of health, but for life itself.—*Lancet*, *Sept.* 29, 1866, *p.* 350.

19.—RAW FLESH AND BRANDY IN THE TREATMENT OF PHTHISIS.

The method of treating consumptive diseases by raw meat and alcohol appears, according to M. Fuster's statements, to have been attended with wonderful results. It has now been tried in no less than 2000 cases, and in nearly all successfully. The patients increase in weight to the extent of two, three, four, or six kilogrammes in the course of two or three weeks. M. Fuster recommends the adoption of his treatment for the following maladies:—Advanced anæmia, the last stages of ague, typhus and typhoid, leucocythæmia, albuminuria, and diabetes, and also in cases where there has been great loss of blood or seminal fluid.—*Lancet*, *July* 21, 1866, *p.* 70.

20.—ON THE INFLUENCE OF SEX AND AGE IN DETERMINING THE LIABILITY TO ASTHMA.

By Dr. HYDE SALTER, F.R.S., Physician to Charing Cross Hospital.

[Since the publication of Dr. Salter's work on asthma he has preserved very copious notes of a hundred and fifty cases of the disease.]

The first point on which my notes furnish information is *Sex*. In my work on asthma I have stated that men are more liable to it than women in the proportion of two to one. My subsequent experience has been a remarkable confirmation of this estimate. Of 153 cases, 51 I find are females, and 102 males, which is exactly two to one. This is too striking and uniform a proportion, and my numbers are now too large, for it to be fortuitous. Why then, one cannot help asking oneself, are men twice as liable to asthma as women? The answer to this question—the only possible answer—is one bearing directly upon our notions of the etiology of the disease; it must be because the causes of asthma are such as men are more exposed to than women. The different views that have been entertained with regard to the etiology of asthma fall under two principal heads—one, that asthma has a local cause in the lungs; the other, that it depends on a fault in the general nervous system. That that sex which is the most exposed to the vicissitudes and inclemencies of the weather—wet, cold, and night-work—should be twice as liable to asthma as the other, tells strongly in favour, as far as it goes, of the former of these views; for the agencies to which men are exposed, in contradistinction to women, are exactly those which tend to give rise to inflammatory conditions of the respiratory mucous membrane.

I have noticed another striking fact with regard to sex quite consistent with this. Twenty-three of my patients were between twenty and thirty years of age; of these 9 were males, and 14 were females. Between fifty and sixty years of age I had 19 patients; of these 16 were males, and 3 were females. Thus at the period of life at which the lungs are the least likely to be organically affected, but at which the nervous system is more excitable, the females greatly exceed the males—in the proportion of 3 to 2; while at the time of life when organic disease of the lungs is likely to exist, the males greatly exceed the females—in the proportion of 5 to 1.

Age.—In relation to age, my notes give information on three points—the age of the patient at the time of my taking the notes; the age at which the disease first made its appearance; and, as a result of these two, the length of time the disease has existed.

The first point, the age of the patient, is not an important one, and has very little direct instructiveness. My cases merely show that asthma comes under the physician's care at every period of life, and that the largest number of cases are in what may be called middle life—from twenty to fifty. There are more asthmatics of ages between thirty and forty than any other equal period. On each side of these ages—before thirty and after forty—the numbers fall.

A much more interesting and important point is the time of life at which asthma makes its appearance. On this point my cases show the following facts:—That, dividing life into equal intervals of ten years, a larger number of cases take their commencement in the first ten years of life than in any subsequent equal period; that childhood is of all ages the most prolific of asthma. After childhood there is a sudden fall; during adolescence much fewer cases declare themselves. But from this there is a gradual rise up to forty. Thus, the number of cases in which the disease commenced between ten and twenty was 20; between twenty and thirty, 23; and between thirty and forty, 27. From forty to seventy again there is a regular but rapid fall. Thus the number of cases commencing between forty and fifty was 16; between 50 and sixty, 9; and between sixty and seventy, 3. We should naturally expect that the time of life at which asthma was apt to show itself would be closely connected with the causation of the disease; and so I think it is. For I arrive, from the foregoing figures, at the following conclusions:—

1st. That the time of life the most prolific of asthma is the time of meascles, of whooping-cough, and of infantile bronchitis.

2nd. That adolescence furnishes comparatively few cases, because the diseases of childhood, so apt to lay the foundation of it, are over, while the wear and tear and hardships of life, and the deterioration of the body produced by them and by time, have not commenced.

3rd. That from this time exposure and hardship and time begin to tell, and show their influence by the increasing asthma-rate reaching its maximum at middle life. But, it may be asked, why should the tendency for asthma to show itself increase up to forty, and then diminish? Why should it not go on increasing as life advances, especially as we know that the tendency of catarrhal and other agencies to produce inflammatory conditions of the respiratory mucous membrane does increase up to the very end of life.

This brings me to the fourth point, which is this: That this diminishing probability of asthma making its first appearance after middle life shows that it does not follow the same law as bronchitis, and that there is something necessary for its development besides vascular change in the bronchial tubes and other organic lung mischief. The other thing is doubtless the asthmatic tendency or idiosyncrasy; and the way in which the necessity of the asthmatic idiosyncrasy for the production of the disease accounts for the diminishing probability, as life advances, of its making its first appearance is this: As every year is added, an individual is decreasingly likely to be exposed *for the first time* to the exciting cause of the disease; if any one has the predisposing cause—the asthmatic tendency—within him, it is not likely he will travel far through life without the exciting cause presenting itself and bringing the disease into activity, and those only can reach advanced life, without becoming asthmatic in whom either the asthmatic tendency is *nil* or feeble, or who have fortuitously escaped circumstances calculated to call it into activity. Such a number must, according to the doctrine of chances, be a constantly decreasing series. The diminishing number of cases is, in fact, an exact measure of the diminishing probability of a person with the asthmatic tendency postponing his first exposure to exciting causes to so late a date.

There is nothing in relation to asthma about which more misconception prevails than the time of life at which it is apt to occur. It is commonly thought to be a disease of old age, and we frequently hear the expression "as asthmatic as an old man." I believe there are two reasons for this error. One, that asthma, if it is not lost comparatively early in life, or if it comes on in middle life, is generally never lost, and therefore exists in old age; and thus many old people are truly asthmatic simply because they have never ceased to be so. The other reason is that chronic bronchitis—undoubtedly a disease of advanced life—is often mistaken for asthma: an old man coughs and wheezes and spits, and is said to have the asthma; but he has really chronic bronchitis; and although the bronchitis may have a little bronchial spasm superadded to it, or even a great deal, still it is essentially and substantively bronchitis.

The error of imagining that asthma is not a disease of early life is one into which. as I have shown, even medical authorities have fallen. But my cases furnish abundant evidence that asthma may, and frequently does, occur very early in life; that there is no time of life that is free from it; and that it may occur even in infancy. A striking example came under my notice a few weeks ago. I was sent for by my friend Dr. Martyn, of Knightsbridge, to see a patient in consultation with him. I found a child in its mother's arms apparently strangling. The child was livid, and its efforts to get air were so violent, and at the same time so ineffectual, that it did not seem as if the struggle could be long continued. Yet its parents told me that it had been in this state, on and off, for six weeks, the wheezing being some times so loud that it could be heard from one end of the house to the other; and, what was more strange, while I was looking at the child, and watching its violent efforts, it made signs that it wanted certain toys to amuse itself with, and actually did amuse itself with them in the midst of such struggles for breath—a sure sign that it had become accustomed to them. From the choking character of the dyspnœa, and the way in which the child threw its head back, I fancied at first that the spasm must be laryngeal; but the examination of the chest and the history of the case showed that the bronchial tubes were the principal seat of the difficulty, though I am inclined to think there was at times laryngeal spasm as well. While I was watching the child, and wondering how it could amuse itself with its toys in such a state, the difficulty of breathing began to abate, and in five minutes there was hardly a trace of it. This, its parents told me, would often be the case. Now there could be no doubt that we had here a case of genuine spasmodic asthma in a child of fifteen months old—indeed the child was only thirteen months and a half old when its symptoms commenced.

Exactly one quarter of my cases (38 out of 153) occurred under ten years of age, and, of these, five were under eighteen months. In four-fifths of the entire number the disease appeared under forty years of age. The most advanced age in which it appeared was sixty-nine—I had one patient of seventy who had only had it a year.

Length of standing.—The longest time that asthma had existed in any of my cases was sixty-four years. This period had been reached in two instances; in one case the patient's age was sixty-nine, and he had become asthmatic at five; the other was seventy years old, and the asthma began at six.

There is no rule that the oldest cases have had the asthma the longest or the youngest the shortest time. Cases of the same age will have had their asthma very different times, while cases of equal standing will be found at various ages. Thus two of my cases aged seventy years had had their asthma one a year and one sixty-four years; while a third of the same age had had it just half his life, or thirty-five years. Again, two of my cases of equal standing, both having had asthma twelve years, were one aged sixty-nine, and the other fifteen—one an old man and the other a young girl—yet their asthmatic age was the same. In this way there is every variety—no rule. Twenty-seven of my cases were of upwards of twenty years' standing, and of these, two had suffered from asthma thirty-four years, two thirty-five years, one thirty-seven, one thirty-nine, one forty-three, two forty-five, one fifty-three, and two sixty-four. Such numbers as these show how little tendency the disease has to shorten life.—*Lancet, July* 28, 1866, *p.* 90.

21.—ON THE IMMEDIATE EXCITANTS OF THE ASTHMATIC PAROXYSM.

By Dr. Hyde Salter, F.R.S., &c.

In considering the causes of asthma, it is necessary to bear in mind the broad distinction between the causes of the disease and the causes of the *paroxysms*. Sometimes these two are the same—that which has laid the foundation of the disease will produce the subsequent attacks. Thus asthma often takes its origin in bronchitis, and whenever bronchitis subsequently occurs it brings an attack of asthma with it. But much more frequently the cause of the disease and the causes of the paroxysms have nothing to do with one another, are quite distinct; that which produces the disease not producing the paroxysms, and that which produces the paroxysms not producing the disease. Thus measles may have laid the foundation of the disease, but it never occurs again to produce the paroxysms; on the other hand, the presence of ipecacuanha powder in the respired air may produce the paroxysm, but it could not produce the asthmatic tendency.

The provocatives of the asthmatic paroxysm may be divided into those which are applied to the air-tubes themselves (or, at any rate, those which affect them primarily), and those which are applied to some remote part, and affect the air-tubes secondarily. These last always reach the bronchial tubes by a nervous circuit—ganglionic or cerebro-spinal.

Those which are applied directly to the bronchial tubes, or affect them primarily, are of three very distinct kinds: *a*, things inhaled; *b*, an inflammatory and therefore morbidly irritable condition of the bronchial mucous membrane; *c*, the presence of a blood that, not necessarily morbid, nevertheless, in the particular pulmonary system of the asthmatic, constitutes a source of irritation: as, for example, what we may call the *sanguis cibi* —the blood after a meal.

Those irritants which produce asthma by acting on some remote part may either be such as are applied to the *peripheræ* of the nervous system—e.g., an indigestible meal, a loaded rectum; or such as affect the brain itself; and these last may either be physical in their nature—some morbid condition of brain—or psychical.

My notes furnish examples of all these provocatives of asthma, some of which are very curious.

In the majority of cases the provocatives of the attacks belong to more than one class: it is the exception to find that *only* local irritants, or *only* psychical causes, or *only* sources of reflex irritation, may give rise to the fits. We commonly find in each case such lists as these; "the effluvium of hay, a late supper, the catamenial period;" or "mental excitement, nuts, fog;" or "thundering weather, wine, the smell of a lucifer match;" in which examples the irritants belong to entirely different groups —one nervous, one bronchial, one peptic. And I think that this fact is not without its indicativeness; I think it shows that the asthmatic tendency consists, in its essence, in a *general* and not a *special* irritability of the nervous system—not in an idiosyncrasy that resents this or that particular thing, but in a general tendency to be irritated.

There are many excitants of asthma whose *modus operandi* is not very clear, and with regard to which the questions at once arise in the mind—How do they act? To what class do they belong? Let me endeavour to answer these questions with regard to some of those doubtful irritants, and then it will be seen hereafter why I assign them the place among the provocatives of asthma I do.

How does *cold* act? I believe in three ways. 1st. As a local irritant, just as damp or fog acts, the cold inspired air at once producing the bronchial spasm. 2nd. As a stimulant to the cutaneous surface, acting reflexly *viâ* the centripetal spinal nerves. In no other way do I conceive we can explain the *immediate* production of asthma by cold to the feet, &c. 3rd. By producing catarrhal bronchitis. In the first two cases the cold acts at once; in the last remotely, not perhaps for days. In the first two a certain amount of cold inevitably produces the asthma; in the last it entirely depends on whether it gives rise to "a cold;" if not, the greatest depression of temperature may be indured without the slightest injury.

How do *heat* and *thunder* act? I believe by producing greater general irritability of the nervous system, and making it more sensitive to sources of disturbance of any kind whatever.

How does *over-exertion* act? In two ways, I think. 1st. By taxing the respiratory organs, and generating a certain amount of respiratory arrears and consequently of pulmonary congestion; and perhaps by the rapid passage of fresh and cold air over the bronchial mucous membrane. 2nd. By producing nervous excitement and exhaustion. In which of these two ways it acts in any given case is shown by the time in which the result takes place: when the over-exertion produces the asthma at once it acts in the two former ways, or both; but when the asthma does not occur till some time after—as, for example, when an asthmatic boy always has an attack *the day after* a cricket match,—it is only in the last way it can act.

How does *laughing* produce asthma? There is no doubt of the fact that it *does* so in a great many cases. In at least a dozen instances patients have spontaneously told me that laughing is always dangerous to them; and I have known several cases of hay fever in which, although the individuals could laugh with impunity at other times, yet during the hay season they never dared laugh lest it should make them asthmatic. I believe it possesses this tendency by producing a condition of extreme expiration. There is no doubt that this favours asthma. I have known many patients who could always make themselves decidedly asthmatic (when the thing was hanging about them) by making two or three prolonged expirations, and emptying the chest of air; the same kind of thing that a person does when you tell him to wheeze. Why is this so? Why should prolonged expiration favour asthmatic contraction of the air-tubes? I suppose it is on the same principle that a very deep inspiration will break through and destroy it—a fact which I have heard affirmed by a hay asthmatic: I suppose it is that the expiration, relieving the tube of the distending traction of the expanded lung, *allows* it to contract, ceases to oppose the tendency to contract, and so allows the spasm to assert itself unopposed, and get more and more complete control over the tube. Anyone who has watched the act of laughing will have observed that it consists principally of a series of short expirations. But not only does laughter produce an expiratory condition of the chest, but it very much suspends respiration for the time, as is shown by the breathlessness and congestion of the face it induces. Now anything that, by suppressing respiration, produces congestion of the face, produces also congestion of the lungs; it is possible, then, that laughing may tend to produce asthma in this way also. I have thought, too, that laughing may tend to produce asthma in another way. It is an involuntary and excito-motory (though not reflex) act: may not the condition into which the nervous system is thrown in laughter tend to favour the development of excito-motory action in general?

Lastly, how do *coughing* and *sneezing* tend to throw the bronchial tubes into a state of spasm? I think by throwing the air passing through them into violent motion and vibration. This must be somewhat similar to the production of asthma by the inhalation of mechanical irritants: it is, in fact, a form of mechanical irritation. I have known talking produce asthma, and I fancy it must do so in a similar way by the irritation which the laryngeal vibrations, propagated down into the lungs, offer to the morbidly sensitive air tubes. Coughing, especially in long fits, may also act by producing pulmonary congestion.

I think this examination of the *modus operandi* of these provocatives of asthmatic spasm is worth the time and trouble spent in discussing them; for unless thus explained they possess no instructiveness and contribute nothing to the pathology of the disease.

The largest group of all is furnished by materials inhaled. These may be divided, I think, into three classes:—1, particular kinds of air; 2, chemical and mechanical irritants; 3, certain (or rather, perhaps, I should say very *uncertain*) animal and vegetable emanations.

1. *Some inappreciable quality of air.*—This, one of the commonest exciting causes of asthma, is expressed in different ways and different degrees in different cases. In some cases change of air is the great thing. This is often felt at first going to a place,

and only at first; the first night or two will be asthmatic, however well the place may afterwards agree. In another class of cases asthma is sure to come on if certain specified places are visited; they may be few or many, but whenever they are visited the asthma is sure to supervene. In many of my cases the individual places which bring on the asthma are mentioned; in one it always came on an hour or two after going into the country, in another going to Malvern always brought it on, in another Hampton Court, in another Denmark-hill, in another it was Brighton, in another a particular house at Matlock.

In another set of cases change of weather, without change of place, seems sufficient to excite the asthma; even change of wind. In some of my cases the special winds are mentioned: in one it will be east wind, in another south-east, in another any high wind, in another any cold wind.

And this brings me to another class of cases, in which the great thing that induced the asthma was cold—cold frosty weather, change of temperature from one room to another, being out in the night air. But I am not sure whether the cold air produces the asthma in these cases as the respired medium, irritating the air-tubes, or in the other ways which I have already mentioned.

In other cases a close air—as in a crowded room, a church, a railway carriage—is mentioned as that particularly apt to give rise to the asthma.

2. *Chemical and mechanical irritants.*—Dust is, of all mechanical irritants, that which I think most frequently recurs in my notes. Next to that comes smoke; the burning of a lucifer match would in many cases always bring on an attack. In one gentleman the smell of camphor would always bring on asthma, and in another the smell of sulphur. In another case stifling gases of any kind. One lady always had her symptoms induced by the smell of paint, and another whenever a bed was made in the room where she was. Dust from hay was particularly offensive in one instance, so that the patient could never go into a hayloft. Sneezing, speaking, and coughing must, as I have already mentioned, be considered mechanical irritants; they frequently occur in my notes.

3. *Animal and vegetable emanations.*—Perhaps the most curious, I might almost say incredible, incidents to be found in my notes, relate to the production of asthma by certain animal and vegetable emanations, especially animal. At the time of the publication of my work on asthma, I was acquainted with only two animals the effluvium from which would give rise to asthma—cats and rabbits. Since that time I have met with cases in which the effluvium from horses, wild beasts, guinea-pigs, cattle, dogs, hares, would immediately give rise to

a paroxysm. One patient, the proprietor of a well-known equestrian establishment, always had his asthma brought on by the presence of his horses ; consequently he was continually asthmatic. He had no suspicion of the real cause of his symptoms till he made his fortune, and retired from business, when he almost entirely lost them ; but if at any time he goes back to his old haunts among the horses, his old trouble immediately shows itself. By giving up his avocation at any former time of his life he would have probably lost his disease.

I might mention many similar cases from my notes. In one the patient when a boy was never able to keep rabbits in consequence of the effluvium from their hutches always bringing on asthma when he went near them. In another, a lady was always unable to visit the Zoological Gardens without being rendered asthmatic, especially was she unable to go into the animal houses. In one case a great variety of animals had the power of giving rise to asthma—horses, rabbits, sheep, oxen, and dogs; this gentleman could never go to a horse-show or dog-show without becoming immediately asthmatic and being compelled to leave. Three of my patients belonged to a family in which this peculiarity existed for three generations, and was evidently hereditary. The grandfather was affected by cats, and could always find out by his breathing if there was one in the room. A grandson, who was also asthmatic, always had an attack brought on by the smell of guinea-pigs. A nephew could never go near horses without being rendered asthmatic nor could he be in a room where those were who had been riding. He was a country gentleman and frequently anxious to attend agricultural meetings, but he was unable to do so from this circumstance. Another nephew told me a curious thing of himself, which, if correct, is perhaps more curious than any of the other facts. On two occasions, when staying at a friend's house in the country, he was attacked with asthma, and found in both instances that there were deer feeding at the time immediately beneath his window ; on a third visit, when the deer had been removed to a distance from the house, he was quite free from any asthmatic symptoms.

One of these gentlemen told me of a friend of his, a country clergyman, who was always rendered asthmatic by the neighbourhood of a hare or hare-skin. If he met any of his parishioners on a Sunday who had been poaching and had their booty about them, he could always in this way detect them. When this gentleman was a boy, and studying with a private tutor, a friend put a hare under a sofa in a room where he was, as a practical joke ; the result was an immediate and very severe attack of asthma.—*Lancet, Sept.* 8, *and Oct.* 6, 1866, *pp.* 259, 384.

DISEASES OF THE ORGANS OF DIGESTION.

22.—ON THE TREATMENT OF DIPHTHERIA.

By Dr. W. Newman.

[A direct cure is not to be looked for in diphtheritic affections any more than in the wider division of fevers. We can only hope to tide the patient over a disease more or less severe, and guide rather than to cut short the case. The one essential characteristic of well-developed cases of diphtheria is a distinct tendency to death from general failure of strength].

From the very first onset and recognition of the affection, I would urge that the patient be kept in the recumbent position, and be spared any unnecessary exertion; that he be fed frequently rather than largely; and that the air of the room be often thoroughly changed. Do not, blindly following a mischievous routine, administer aperients; least of all, those containing mercurials. A simple injection of warm water will probably meet every requirement. Nor is it advisable to encourage perspiration by sedative or lowering medicines.

In this, as in all septic diseases, I would ask for constant circulation of fresh air through the apartment. Screen the patient from direct draught, but yet by continuous fire and open windows ensure that he shall not breathe again and again an atmosphere vitiated by his own excretions.

Medical treatment will embrace

1. *Local applications.*—So soon as the exudation is fairly manifested, and the co-existence of vivid redness of the surrounding mucous membrane points to its probably rapid increase, I hold it the best to use at once, as the most efficient application, the strong hydrochloric acid, diluted either with honey or with water, in equal bulk. A piece of sponge tied firmly on a stem of stick or whalebone, and then well saturated with the diluted acid, will be best for the application; and this once made the patient will usually declare himself much relieved. It seems to do away with the burning sensation about the fauces at the cost of a comparatively momentary pain, and furthers the curling up and detachment of the coriaceous layer. Once or twice again it may be well to resort to this potent caustic at an interval of twelve or twenty-four hours, rarely more frequently.

Then I have used as an after application the undiluted Beaufoy's solution of chlorinated soda, also used with a sponge or large brush every four or six hours.

To make any of these applications thoroughly, the tongue must be well kept down with a spatula, or drawn forward out of the mouth.

Gargles are of little value. It is true that they wash out the mouth and the anterior part of the fauces, but they do not go back to the special seat of the disease.

In those not very infrequent cases where fœtor exists from decomposition of the secretions, or from commencing sloughing, I have had recourse to a tolerably strong solution of the chloride of zinc, and I think with decided advantage; and as an addition to the patient's comfort, the permanganate of potash (Condy's fluid), with six or seven parts of water, may be employed to cleanse the mouth from stringy saliva and offensive discharges.

In the earlier cases the nitrate of silver was used as an application, and then totally discontinued. This agent has always seemed to me inefficient as a caustic, apt to deceive the eye in subsequent examinations from the coating which is left behind after its use, and unsatisfactory in any influence for good in the removal of exudation or effect on the tissues.

Of external applications I have little to say, other than to deprecate very positively any resort to those which may remove the cuticle or damage its condition. More than once I have met with cases where after the local use of blisters, or liniments of liq. ammoniæ, the irritated surface has been speedily covered with the characteristic lymph exudation, but in no one instance has even temporary benefit appeared traceable to such means.

For the comfort of the patient, and indeed with some actual value, I think it well to apply an evaporating lotion of spirit of wine to the swollen and infiltrated structures beneath the jaw.

2. *Internal remedies.*—The class of internal remedies from which the most good may be hoped is that of tonics, and from many conditions which are present in severe diphtheria preference should be given to preparations of iron. These, in one form or other, I would give even from the first declaration of the disease, not waiting until increased pallor of integument and advancing debility make such agents palpably needful.

The ordinary tincture of the sesquichloride is for several reasons most to be advocated, not merely from its effects on the circulating fluid, and from its powers as a general nervous tonic, but also from its presumably good effect on the affected tissues as a direct astringent.

Again, it would seem that all compounds containing and readily parting with chlorine are of service, so the chlorate of potash may be given in conjunction with the iron; or, for the good purpose of combining in one, an efficient local application, and a means of introducing chlorine readily into the system, the formula given below may be employed with advantage.

℞. Potass. chloratis ℥ij; acid hydrochloric. ℥j; aq. destillat. ʒij.

Mix the acid and water together first; add the mixture to the chlorate of potash, and keep the whole in a dark place.

ʒij. of this solution to be mixed with a pint of water.—Dose one or two tablespoonfuls, according to age, for internal use; or as a wash for mouth or gargle.

Several of the worst cases of diphtheria which I have seen have seemed to owe their recovery to the steady use of this compound; especially one where life for days together hung by a thread, and where a huge coherent cast of the pharynx and back of the throat was expelled.

Cinchona, as the decoction or tincture, I do not think of material value; and quinine, although I have commonly given it where there has been much loss of appetite, excessive perspiration, and so forth, has seemed to carry with its use only an indirect good.

3. *Food and Stimulants.*—These play no unimportant part in the medical dealing with so serious a malady. Small quantities of nutritious and easily digestible food should be given at very short intervals; and no existence of feverish symptoms should for one moment be deemed warrant for confining a child to "low diet." I believe that, humanly speaking, very many deaths from diphtheria have resulted from inattentive nursing, and from an unwillingness to press on a listless sufferer the prescribed supplies of food.

New milk is of very much value, and may be used in large quantities, and, at intervals with this, beef tea, essence of beef, or other nutritious foods. Farinaceous compounds are least to be trusted to.

Sometimes, and indeed as if it were an integral part of the affection, there may be absolute repugnance to food; a condition, if lasting beyond some short time, which renders ultimate recovery almost impossible. Yet here even much may be done by quiet perseverance, and, failing other means, nutrient enemata should certainly be tried.

Stimulants should be used early and without hesitation; as in cases of fever, the first-noted feebleness of the heart-beat, or softness and rapidity of the pulse, furnish full reasons for their employment. Wine is probably to be preferred to other alcoholic compounds.

Accidental complications, very apt to occur, will call for changes of treatment, and a wearisome illness cannot throughout be treated by the same remedies; but the declared principles hold good that the disease is eminently asthenic in type, and that no lowering agencies are to be deemed admissible.

Extension of local mischief to the larynx may occur in the most simple cases, and certain expedients should at once be tried. Emetics (the sulphate of zinc, for example) may be given, and the child made, as in croupy attacks, to breathe an atmosphere loaded with warm vapour, with other means which may promise relief.

If these, however, fail, tracheotomy may be suggested; in four well-marked cases I have myself done the operation, substituting thereby an easy for a most terrible death, and giving some hours more of life when life was on the point of passing away. The details of the necessary operation may be found in any surgical work. Cautiousness and slowness are the two most necessary points to be observed in performing it. Yet although I would in a suitable case propose and carry out the same procedure, it would still be with the fear that recovery would be most uncertain; not from the local throat mischief, or in any sense from the operation itself, but from the widespread taint of the general system. Relief may be promised with fair confidence, painless death will take the place of an agonising struggle for breath, but a very small per centage of cases ultimately do well.

Supporting treatment, enforced and long-continued rest, with change of air, form the best rules for dealing with the consequences of diphtheritic affections; loss of nerve power is but slowly repaired, and though, in the great majority of cases, recovery may be certainly expected, it is only after a very prolonged convalescence.—*St. Bartholomew's Hospital Reports, Vol. II*, 1866, *p.* 42.

23.—HYDATID OF THE LIVER, TREATED SUCCESSFULLY BY THE INJECTION OF THE EXTRACT OF MALE FERN INTO THE CYST.

By Dr. F. W. Pavy, F.R.S., Assistant Physician and Lecturer on Physiology at Guy's Hospital.

Harriet V., a woman of pretty healthy appearance, aged 21, admitted into Mary ward under the care of Dr. Pavy, Oct. 4th, 1865. When three years old she was squeezed against a wall by a cart wheel, which struck her somewhere on the right side of the chest. No rib was fractured, and she soon recovered from the accident. About six years ago the patient noticed a slight swelling in her right side, which has since continued gradually increasing in size.

On examination, a large deep-seated tumour was to be noticed occupying the right hypochondriac region, and extending considerably beyond, both above and below. Its boundary could be clearly defined inferiorly. It caused a considerable bulging of the ribs on the right side, and the right mammary gland was raised about three-quarters of an inch above the level of the left.

Fluctuation was apparent. Dulness extended as high as the lower border of the second rib on the right side.

The case was diagnosed to be one of hydatid tumour of the liver. The relationship that is agreed upon by helminthologists to exist between the hydatid and the tænia, and the known effect of the extract of male fern upon the latter, suggested to the author the treatment adopted. The extract is not miscible with alcohol or water, but it was ascertained that a liquid sufficiently thin for passing through a fine canula was to be obtained by admixture with a little potash.

Nov. 6th. A fine trocar and canula were introduced into the tumour by Mr. Durham, and about four ounces of a limpid colourless fluid allowed to escape, in order to diminish the tension of the cyst. A liquid consisting of half a drachm (by measure) of the purified semi-fluid extract of male fern, half a drachm of liquor potassæ, and six drachms of water, was then injected into the sac, care being taken throughout to prevent the entrance of air. The fluid removed was examined, and found to be non-albuminous, charged with a large quantity of the chloride of sodium, and to contain hooklets of the echinococcus. At the introduction of the trocar the patient complained of experiencing a considerable amount of pain, which she referaed to the lower part of the abdomen. Some febrile excitement, vomiting, and purging followed, but there was no evidence of peritonitis.

10th. On percussion it was found that dulness did not extend so high in the chest on the right side by one rib as previous to the operation.

16th. The patient was allowed to get up.

20th. The tumour was found to be much diminished in size. It was much softer, did not extend so low down in the abdomen, and was much less distinctly circumscribed. The chest was resonant on percussion as low as the space between the fourth and fifth ribs.

29th. The circumference over the most projecting part of the tumour before the operation was 34¼ inches; to-day it is 31¾ inches; showing a reduction of 2½. Tumour very soft, and its lower border not to be defined as formerly. The patient, being well, was allowed to leave the hospital.

A fortnight and again a month afterwards she was seen, and found to be progressing satisfactorily.

May 10th, 1866. Since she was last seen the patient had suffered from an attack of rheumatic fever with heart complication, and bronchitis. She had been in no way troubled with her side, and her circumference was now thirty inches. No swelling was perceptible to the eye, but a hardness remained in the hypochondriac region.

The inference to be drawn from the result in this case is that the injection of the extract of male fern caused an immediate destruction of the life of the hydatid without the production of suppuration, and that a rapid absorption of the fluid element of the cyst afterwards took place.—*Lancet, Sept.* 1, 1866, *p.* 234.

DISEASES OF THE URINARY ORGANS.

24.—ON PAROXYSMAL HÆMATURIA.

By Dr. F. W. Pavy, F.R.S., Assistant Physician and Lecturer on Physiology, Guy's Hospital.

[During the last two years Dr. Pavy has seen two of the cases to which he has given the above name. The first patient was a gentleman of middle age, who said that a slight exposure to cold was usually followed by the passage of urine of a more or less porter-like colour. The urine was found to be opaque from lithates, and also to contain albumen. The microscope also showed the presence of oxalate of lime octahedra. Between the attacks, beyond a deposit of crystals of lithic acid, the urine was perfectly natural.]

My patient informed me that during the last year or two he had frequently suffered from similar attacks; that he had been under different medical men, and had taken quinine and various other remedies for his complaint. It had been ascertained that there was no stone in his bladder; and it was not exercise but exposure to cold that brought on his attacks. He was comparatively free from them in the summer. There was no periodicity about their recurrence of the character belonging to ague; and he had never, as far as he knew, been the subject of that complaint. Riding or driving on a cold day, if his feet got cold, sufficed to bring them on; and on this account he had been obliged to give up riding with the harriers, a sport he had been hitherto frequently accustomed to join in. Going out of doors and standing for a few minutes in the cold air, although well clad, had been sufficient to bring on an attack. If he could only keep his hands and feet warm, he told me, he felt safe; but as soon as these parts got cold he was almost sure to have an attack. His countenance was somewhat sallow, and he had been losing strength and flesh.

With this history I prescribed a tonic and some extract of belladonna, and urged him to wear fur gloves and over-boots when he went out of doors in the cold. Adopting this plan, he passed through the remainder of the winter of 1864-5 almost free from attacks, and improved in health and strength. If such had not taken place, I had determined to advise him to change his residence during the winter to a warmer climate; but this was, if possible, to be avoided, as it would have involved with him a considerable pecuniary sacrifice.

During the past winter he has got about much more freely, and in the early part of it was almost free from attacks. In the latter part of it, however, he had been less careful of himself, and had suffered accordingly. Sometimes he has averted an attack by going indoors directly he felt it coming on, sitting in front of a fire, and drinking something warm. In April last he wrote to me as follows:—

"My last attack was brought on by riding in the cold, although I did not feel it but very little until I got to the end of my journey, was sitting in a warm room, and had a slight shivering. Had a glass of hot sherry-and-water, and, in about ten minutes, I wanted to make water. Did so, and it was the colour of porter. In about ten minutes again, the same irritation as before, and made about a wineglass of water, more blood colour, and the same in about another quarter of an hour. Then I became very warm and feverish, and I did not make water again for more than two hours; when I did so it was quite clear. On Tuesday morning last, when I got up at half-past eight,. I washed in cold water, and sat down to breakfast. My feet turned cold, and I felt a little queer, so I turned to the fire, and drank a cup of hot tea, and got warm. When I made water about two hours after, it was the colour of dark ale, not quite so dark as porter. I always use myself to wash in warm water. I still have the cough when attacked, and until the perspiration comes on it does not leave me. After this I feel faint, and sometimes almost faint off. I always feel cold at the chest before passing blood."

The other case is that of a gentleman, rather beyond the middle period of life, who was brought to me by Mr. Acton in December last. The urine passed by the patient at my house was natural in colour, and free from albumen; but I was informed that it was occasionally for one or two micturitions highly charged with blood. Mr. Acton had several times seen it in this state. The history disclosed the same connexion between the attack of hæmaturia and exposnre to cold which existed in my other patient, and I expressed the opinion that I considered it to be a perfectly parallel case.

In the "Medico-Chirurgical Transactions" for 1865 cases are recorded by Dr. Harley and Dr. Dickinson of intermittent hæmaturia. Rayer, in his "Maladies des Reins," speaks of intermittent hæmaturia, and refers to reports of two cases that were cured by the administration of quinine. Dr. Harley says, in reference to one of his cases, that the patient replied, when asked what was the matter with him, "I can't tell you; but each time I get cold hands or cold feet, I pass bloody urine, while my urine at other times is perfectly healthy." This patient was a Londoner, and had never suffered from ague. Dr. Harley's other patient was the subject of malarial poisoning, and had been obliged to return from the West Indies in consequence of repeated attacks of intermittent fever. Dr. Dickinson gives the particulars of a case under Dr. Fuller's care in St. George's Hospital. The patient had twenty years previously been the subject of a tertian ague, but had completely recovered from this, and remained in good health till 1859, when he began to suffer from attacks of hæmaturia, coming on after exposure to cold. He had been frequently an inmate of St. George's Hospital, and was still suffering from the complaint. Within an hour or so after exposure to cold he would have an inclination to pass water, and would void a considerable quantity of black turbid urine, like porter. The urine would retain this character for two or three times of passing, and then resume its natural appearance. Between the attacks the urine was devoid of albumen, and natural. Dr. Dickinson also gives notes of two cases, which had been furnished to him by Dr. George Johnson. In both of these there was no history of ague, and the attacks occurred only after taking a chill. The urine at other times was normal. A fourth case is mentioned, the report of which was furnished to Dr. Dickinson by Dr. Frederick Cock, of Westbourne-park terrace. In this, however, the attacks were of longer duration and less frequent recurrence. After suffering for two or three days from a "severe cold," the urine of the patient would assume a dark colour, like porter, and become highly albuminous. This dark colour afterwards diminished, until in a fortnight or three weeks, it had disappeared. The albumen diminished simultaneously, and in its turn passed completely away.

The cases narrated by Rayer seem evidently to have been cases of intermittent fever, accompanied with hæmaturia as one of the concomitants arising, it may be considered, from the congestion to which internal organs are submitted during the occurrence of the paroxysm. It is stated by Dr. Parkes that albumen is found in the urine during the fit in a considerable number of cases of ague : in one-tenth according to Finger ; in one-fourth according to Martin Solon. Blood (he further says) in some quantity, and renal cylinders, are seen about as frequently as albumen, and occasionally chronic Bright's disease is a consequence of ague.

The cases, however, to which this communication is intended to refer are cases of altogether a different description. There is no evidence that the paroxysms depend upon the existence of any blood poison. There is no regularity in their recurrence.

They are brought on by exposure to cold, and exposure to cold only. When the hands or feet get chilled, the patient is seized with shivering, has a general feeling of discomfort, and begins to pass immediately urine more or less dark-coloured from the presence of blood. It may be assumed that there is no organic disease of the kidney, as the urine, except during the attack, is healthy. An alteration in the quantity of the blood, or in the state of the blood-vessels, must constitute the immediate precursor of the escape of blood from the kidney. The rapidity with which the effect upon the urine follows the exposure to cold scarcely leaves ground for supposing that an altered condition of the blood could be set up to occasion it. We have only, therefore, an altered condition of the blood-vessels to fall back upon, and physiology teaches us how rapidly and readily impressions may act through the nervous system, and modify the circulation and nutrition of a part. It is a matter also of the most common observation to witness exposure to cold producing a modification of the circulation in some locality or another. And dispositions of different individuals vary; so that in one the impression of cold, through getting damp feet, for example, should be followed by a coryza; in another, by bronchitis; in another, by pleurisy, or, may be, an inflammation of the lungs; in another, peritonitis; in another, inflammation of the kidney; and so on. There are varying degrees of susceptibility appertaining to different parts to be impressed by exposure to cold in different individuals. In the affection that is being commented upon, it may be considered that an unusual susceptibility of the kidney to temporary congestion from exposure to cold happens to exist. The effect, it may be further considered, is determined by the impression resulting from the exposure acting through the spinal and sympathetic systems on the muscular coat of the blood-vessels. Taking this view, the principle I acted upon in treatment was, first of all, by suitable coverings or clothing, to diminish the risk of the patient getting chilled, and next to fortify the system by tonics, and render it less impressionable by a narcotic.—*Lancet, July* 14, 1866, *p.* 33.

25.—ON DIABETES.

By Dr. G. Owen Rees, F.R.S., Physician to Guy's Hospital, and Lecturer on Medicine at Guy's Hospital School.

[Many must doubt whether the plan of treatment usually adopted by the profession be really the best; whether it is really necessary to deprive the patient of all starchy food which must be required for his system the same as for that of a person in perfect health. Moreover, it is not uncommonly the case that the emaciation which progresses on a diet not containing starch ceases to do so when the patient eats freely of amylaceous food.]

Does the circulation of sugar really do mischief? Let us consider this question. Now it is beyond doubt that diabetics may take large quantities of sugar (three-quarters of a pound a day) for weeks without any symptoms occurring. The experiment has been made by many. I myself have performed it, and the patient was very comfortable the while. I have often, too, allowed large quantities of potatoes to be eaten, the diabetic meanwhile looking well and enjoying his diet. The above facts, however, are scarcely so much to the point as those I have observed in patients who have been doing badly while on purely animal diet, and who, when weak and emaciated, have been allowed free use of farinaceous food. The following case is instructive in this respect.

Mr. A——, a farmer, residing in Essex, came to me complaining of extreme weakness, the result of diabetes. He could scarcely walk, and was much emaciated. He had been under strict dieting; no amylaceous food allowed. The urine was of specific gravity 1.040, and he passed about four pints and a half in the twenty-four hours. His extreme weakness and general want of power alarmed me, and as his diet had evidently not done him any service, I told him to eat freely of vegetables of all kinds, and to take as much bread as he liked; at the same time I ordered him alteratives and tonics. Now, if the circulation of sugar in the blood tends to develope the evils consequent on diabetes, great injury would have been inflicted on this man by the use of a natural diet. The result showed, however, that he *needed* vegetable diet, for he began to improve from the moment he indulged in it. He gained weight and strength, and soon became nearly as stout as ever.

His history may be briefly told. He lived thirteen years after I first saw him, during the latter portion of which time he became rather stouter than before he was attacked by the disease. Thirst occasionally annoyed him, but not often. He passed from four to five pints of saccharine urine in the twenty-four hours, the specific gravity of which varied from 1038 to 1042. He was able to farm without much trouble, but considered latterly that he was getting weaker. He died, aged fifty-five, from the effects of an injury to the foot. Sloughing took place, and he sank rapidly. It is my decided opinion, from what I saw of him on his first visit, that he must have inevitably died in a few weeks, if not sooner, had he not been allowed an ordinary diet. He himself felt quite certain that the restricted diet was destroying him.

Now it is not my wish to induce a belief that diabetics cannot live on a restricted diet for a great length of time. Many of them do so—for months, for years. But what I venture to insist on is, that they do better on a natural diet; that saccharine and farinaceous food is as necessary to the comfort and wellbeing of the diabetic as it is to the healthy stomach; that abstinence from starchy and saccharine matter is injurious to the diabetic; and that the circulation of sugar in the blood is not productive of bad symptoms, either immediately or remotely.

It may be argued that diabetic persons have lived for years on a restricted diet, have been moderately comfortable, and have not suffered from the more severe symptoms of the disease. This is quite true; but must we conclude that they have enjoyed this immunity in consequence of a restricted diet? I think what I have just detailed will be sufficient to render it probable that had such persons been allowed ordinary diet they would have gone on far better, and, moreover, not have suffered the inconvenience so constantly felt by those who are subjected to a diet containing scarcely a trace of amylaceous or saccharine matter. I have now had long experience in the management of diabetes, and, from what I have observed, I have resolved never again to countenance the use of the strict diet which has been so vaunted as necessary to the wellbeing of diabetics. In some cases it is very decidedly injurious, as shown by the instance quoted above.

I scarcely care to deal with the theories by which this question of dieting diabetics has been defended, for the reason that all theories must propound error when they are opposed to facts absolutely determined by long experience. At the same time I can well understand the diabetic watching his own case, and becoming pleased that less sugar is passing from him; and can well imagine his feeling satisfied that some impress has been made on the disease because the two prominent symptoms—viz., the large quantity of urine passed, and the high specific gravity, appreach nearer to the healthy standard. Histories of the sad manner in which this fool's paradise is suddenly broken in upon by death are common enough.—*Lancet, July* 7, 1866, *p.* 4.

26.—ON THE TREATMENT OF DIABETES.

By Dr. F. W. Pavy, F.R.S., Assistant-Physician to Guy's Hospital.

[The following article is elicited by that of Dr. Rees on the treatment of diabetes.]

It is declared by Dr. Rees that dieting is of no use—nay, worse than useless, prejudicial in diabetes. I must confess I had cherished the idea that a great advance had been made during the last quarter of a century in the treatment of diabetes; and this not empirically, but upon grounds of rational observation. I was thus startled at a declaration, which, if true, reduces the question of progress to a mere delusion.

The first case referred to in Dr. Rees' remarks can scarcely, I think, be looked upon as supplying a happy illustration in favour of his proposition regarding the two plans of dieting. The patient was a healthy-looking young woman, who had been for some time the subject of diabetes. She had been under treatment for some weeks previous to falling under the care of Dr. Rees, and had been strictly dieted during that time, receiving it is said temporary relief. She was now prescribed for, and her diet was not restricted. In the course of a few weeks urgent symptoms suddenly set in, and she died.

Now, I do not mean to say that such a termination would have been averted in this patient's case had the restricted dietetic treatment been continued, but I do most firmly believe that the chances of its occurrence would have been materially lessened. My experience has led me to the conclusion (and I have been for some time past in the habit of warning my patients accordingly) that the diabetic who allows the disease—I am speaking of the severe form of it—to run on uncontrolled by dietetic management is not safe from one day's end to another. His tenure of life is precarious in the extreme. At any moment urgent symptoms may set in, and in a very short space of time destroy life. From the extent to which the circulating fluid is charged with sugar, a deviation from its normal condition exists, and deprives it, I believe, of its aptitude for properly administering to the functions of life. That the functions of life are not properly performed under an uncontrolled state of the severe form of the disease is sufficiently manifest on looking to the pitiable condition the patient presents. He loses flesh and becomes reduced to a mere skeleton; he has an indescribable sense of general discomfort in his body; his weakness is extreme; he is frequently the subject of cramps; his virility disappears; he is parched with an insatiable thirst; often possesses an appetite that is difficult to appease; and is troubled and disturbed with the inconvenience of having to rise several times in the night to rid himself of a secretion the production of which is profuse in the extreme. Subdue the disease, which can be often completely, and always partially, done by dietetic management, and a miraculous alteration in a short space of time is the result. His urine falls in quantity; his excessive thirst subsides; his face begins to fill out and to lose its previously pinched-up expression; he acquires strength, increases in weight, and regain his virility. In many cases, friends and patient wonder at the restoration that has taken place in even a week or two's time. It is upon actual experience that what I have stated is founded. In some instances the sugar disappears after a little while altogether from the urine, or, if it does not disappear altogether, may become reduced in quantity to a few grains to the fluid ounce. In the case of such a patient the control of the disease is, I believe, in his own hands, and it is his own fault if he allow it to prove fatal. In this condition he is no longer hinging between life and death, as he was before. He is in the position of a healthy person, save with one exception—the power of assimilating sugar; but as long as this function is not forced into requisition he lives in a state of security. In the process of dieting that is required, certainly in cases where any extreme quantity of sugar is eliminated, half measures prove of no avail whatever. I am frequently told by patients that they have been upon the restricted plan; yet they may have been allowed dry toast or brown bread, or perhaps advised to drink large quantities of milk. Symptoms have thus been kept up which have yielded to a more rigid exclusion of starch and sugar from their diet.

In other instances—and these are especially where the disease occurs in younger subjects—the sugar cannot be reduced below from fourteen or fifteen to twenty grains to the ounce, it having been, under non-restriction, from forty to forty-eight grains to the ounce. Here, although the severity of the patient's symptoms are materially controlled, and, I believe, his life considerably prolonged, yet it too often happens that he slowly declines, and ultimately dies apparently from sheer exhaustion of power, or from lung disease having been set up. It seems as if there is something existing besides what I am convinced is the only fault in many cases—namely, a defective assimilating power over the saccharine element—which strikes deeply at the root of the nutritive changes going on in the body.

It is not to be imagined that the system of dieting is always required to be permanently persisted in. A rigid observance being practised for a time, it often happens that the healthy state becomes thereby so restored, that the patient is ultimately able to take an ordinary diet without any appearance of sugar, or only a little, in the urine. Many cases of this kind have fallen under my observation. The patient at first is suffering from all the symptoms of a severe form of diabetes. He is placed under treatment, and within a few days such an improvement has occurred in his bodily feelings that he has sufficient inducement to steadily persevere in following the recommendations that have been given him. The sugar may have disappeared altogether from the urine. At first, any deviation from the prescribed dietary leads to a reappearance of sugar; but after a while, it may be in a few months or a year, it is found that an ordinary diet may be taken without reestablishing the symptoms of the disease. With persons advanced in life, I look forward to this result as the ordinary occurrence. A lady, upwards of sixty-five years of age, who came to me a fortnight ago from the country, passing urine charged with forty-eight grains of sugar to the ounce, in less than a week passed urine entirely free from sugar. At present the restricted plan is being rigidly carried out, but I look forward to this patient being by-and-by able to return, if not entirely, nearly entirely, to an ordinary mode of living. One thing I could tell her—and this is a matter which affords great relief to patients,—that she had the disease in her own hands, and that it need not curtail her life; which is what, it is my firm conviction, could not have been said if the complaint had been allowed to run on in an unsubdued form.

Here let me state that I hold a quantitative examination of the urine to be indispensable for giving the information about a case that is required. The liquor potassæ test will only give approximate quantitative results, and the copper test, used qualitatively, will react just as strongly with two or three grains of sugar to the fluid ounce as with twenty, thirty, or forty. A quantitative determination being made, it is found, we will say, that only a few grains to the ounce are passed. It is known at once that this is not likely to prove hurtful.

I do not discard or underrate the use of medical treatment in conjunction with the dietetic in diabetes, but without the latter I openly avow that I believe that nothing is to be done by means of the former.

I will not trespass further on your space, but conclude these remarks by referring to a case which I look upon as exceedingly instructive upon the point I have been alluding to. A patient two years ago was the subject of a severe form of diabetes, and, from the loss of flesh that was taking place and other symptoms, made up his mind that he could not live long. In course of a short time this patient's urine under the diatetic and medicinal treatment adopted, became free from sugar, and later he was able to take an ordinary diet without its rendering his urine saccharine. One day he was tempted to take some cider, and drank off a pint at a draught. Almost instantly he began to experience his old sensations, and felt sure the disease had returned upon him. On examining some urine he brought me shortly afterwards, this was found to be perfectly true, and it took some weeks, even under complete restriction, before the sugar disappeared from it again. He, however, is now perfectly well again, and living in an ordinary though careful way, without passing any sugar..—*Lancet, July* 28, 1866, *p.* 106.

27.—ON BRAN BISCUITS IN DIABETES.

By Dr. Arthur Hill Hassall. Senior Physician to the Royal Free Hospital; and Dr. F. W. Pavy, Physician to Guy's Hospital.

[During the past year or two Dr. Hassall has been engaged in making an extensive series of analyses of various articles of food, with a view to determine their fitness or otherwise for the use of the diabetic. One result obtained is very important, viz,; the unfitness of the prepared bran and biscuits now sold for diabetic cases, notwithstanding the statements advanced as to the purity of the bran and its freedom from starch.]

The method of purification now in use consists simply in washing and pressing the bran, whereby, doubtless, a small quantity of starch is removed; but since the starch granules are chiefly enclosed in the cellular tissue of the bran, it is obvious that this process must be insufficient for the removal of the greater part of the starch. That it really is so may be proved in a moment by the simple application of a drop of tincture of iodine either to a small portion of the prepared bran or of one of the biscuits made therewith, when they will instantly become of a deep bluish-black colour. But the quantity actually present may be readily estimated by converting it, through the action of a little dilute sulphuric acid, into glucose or grape-sugar, which may be easily washed away, the sugar estimated, and the quantity of starch present ascertained therefrom. Some analyses made in this manner furnished the following results:—

Unprepared Bran.	Per Cent.
Water	12·04
Oil	2·00
Nitrogenous matter	13·70
Starch	45·65
Gum and digestible fibre... ...	2·95
Fibre	17·66
Ash	6·00
	100·00

Prepared Bran.	Per cent.
Water	10·18
Oil	4·65
Albumen	15·20
Starch	44·50
Digestible fibre	·77
Fibre	19·00
Ash	5·70
	100·00

Bran Biscuits	Per cent.
Water	9·80
Oil and butter	28·30
Albumen	13·96
Starch	28·20
Gum and digestible fibre... ...	5·02
Fibre	9·60
Ash	5·12
	100·00

The above figures demonstrate the accuracy of the statement already made, to the effect that the prepared bran and biscuits now sold *are totally unfit for the use of patients labouring under diabetes.* They serve also to explain the disappointment which is so generally experienced in the use of these biscuits, the urine still continuing to retain a high specific gravity and to abound in sugar. So general has the use of bran biscuits become, that the fact cannot be too generally known that they contain a very large proportion of starch and of matter convertible into sugar.

Now the method employed in the analysis of the samples of prepared bran and biscuits tested, with certain modifications, furnishes us with a means of freeing the bran absolutely from all starch, and of rendering it an admirable material for the preparation of really pure bran biscuits. I have had the process repeatedly carried out on a large scale, and I find that it answers completely. I further find that there is no difficulty in making from the purified bran a "flour of bran" equal in fineness to wheat flour or my "flour of meat," and thus overcoming another objection to the use of the bran—namely, its coarseness.

Lastly, I would observe that the value of this "flour of bran" is greatly increased by its admixture with a portion of my "flour of beef;" and that thus a mixture is obtained more nutritive, and certainly better fitted in every respect for the treatment of diabetes, than anything hitherto devised.

[Dr. Pavy offers as a suggestion whether torrefaction, by which starch is converted into dextrine—an easily soluble substance—would not prove a manageable process for converting starch into sugar, in order to estimate its amount in articles of diet intended for diabetic patients. He says :]

It is an important matter to the diabetic to be supplied with a variety in the food that is to take the place of ordinary bread, so that he may select that which is most agreeable to him, or, as he gets tired of one, may pass to another. Although the almond food, from its composition, is the best adapted for the complaint, yet there are some persons who cannot consume it on account of a dislike to the flavour of the almond, and then the bran or gluten food must be had recourse to. Of these, my experience, from their effects upon the urine, is that the bran is the better for the diabetic of the two. But a great deal depends upon where the bran biscuits are procured, for there are some sold that are really scarcely less objectionable than the common brown bread; and this, although I frequently find it has been recommended to the patient, is scarcely any improvement upon the white bread in ordinary use amongst us. In proof of what I have asserted I will mention a case that but a short time since offered itself to my notice. A gentleman came to me from the country suffering from a severe form of diabetes. He was passing twelve pints of urine in twenty-four hours, containing forty grains of sugar to the ounce. In less than a month, under the dietetic measures and medical treatment adopted, the urine was reduced to nearly a normal quantity, and contained under eleven grains of sugar to the ounce. The patient had been consuming, as a substitute for bread, bran biscuits procured from Mr. Blatchley, of Oxford-street. A fortnight later I found that the urine had gone up to six or seven pints, and contained upwards of twenty-one grains of sugar to the ounce. The medical treatment was the same, and I was informed by the patient that he had adhered strictly to the dietary he had been ordered. I felt sure there must be a cause for this change in what was taken, and I closely questioned my patient upon what had been eaten and drunk at each meal for a day or two past, but failed to discover that he had committed any error. A few minutes afterwards he produced from his pocket a specimen of bran biscuit which had been made for him by a confectioner in his neighbourhood, and asked me what I thought of it. Here, I at once discovered, was the source of mischief I had been looking for. The biscuits were palpably enough, probably for the sake of improving their taste and appearance, contaminated with a quantity of flour; but to my patient they were bran, and, as far as he knew, proper bran biscuits. He at once discontinued them and returned to those he had been previously taking, and immediately, as I had predicted, his diabetic condition abated.

The "flour of meat," introduced by Dr. Hassall, I think supplies what has hitherto been wanted for general purposes in practice, and forms a convenient article of food for the diabetic.

[In another letter in the Lancet Dr. Hassall says:]

Dr. Pavy, in the communication inserted in the Lancet of the 23rd inst., threw out the suggestion that torrefaction of the bran, whereby the insoluble starch becomes converted into soluble dextrine, might afford a simpler and more manageable means of freeing the bran from starch than my method by dilute sulphuric acid.

This suggestion I have put to a practical test, and found that

100 grains or parts of the bran, carefully torrefied at a temperature of 320° Fahr., lost, after being well washed, 24 grains or parts of its weight; that the residue on treatment with iodine became almost black: thus furnishing evidence of the presence of starch in large quantity, and, on analysis, an amount of glucose corresponding to 22 grains of starch was obtained. These particulars afford conclusive evidence against the process by torrefaction, whereby, although a portion of the starch may be removed, a considerable quantity is still retained by the bran.

There is, however, a process which I have had in my mind for some time past, and which recently I have tested practically. It is based upon the conversion of the starch into dextrine through the action of *Diastase.* This method of purifaction is so easy and perfect that it leaves nothing further to be desired; and the results, so for as the colour and composition of the bran are concerned, are certainly superior to those obtained by means of sulphuric acid. Nevertheless, I am of opinion that, in extreme cases of diabetes, the acid method is to be preferred.

It is necessary, in the interest of those who have the misfortune to labour under diabetes, that it should be clearly understood that the analyses given by me of the so-called Pure Bran and Bran Biscuits were by no means exceptional. They were made from samples prepared by one of the most careful makers; and I have never yet met with a bran biscuit which did not abound in starch, while even those sold at the present time, no matter where obtained, still contain starch in very large amount, turning absolutely black with iodine. I wish it also to be clearly understood that I do not lay the least blame of this state of things upon the makers; they, I believe, do their best. The radical fault is not with them, but in the process which they adopt, and which they have been taught to believe was effectual

I conclude this note with an analysis of *Gluten Bread*, which shows how much the amount of starch present in it has been underrated, and how unfitted it also is for the use of the diabetic, except in the mildest cases.

	Per centage composition.
Water	9·30
Oil	1·26
Gluten	60·81
Starch	16·33
Dextrine and undetermined matters	9·70
Ash	2·30
Fibre	·30
	100·00

—*Lancet, June* 9, 23, *and* 30, 1866, *pp.* 640, 698, 719.

SURGERY.

AMPUTATIONS, FRACTURES, DISLOCATIONS, ETC.

28.—ON AMPUTATION ABOUT THE FOOT AND ANKLE.

By Henry Hancock, Esq., Surgeon to Charing Cross Hospital, and Professor of Surgery in the Royal College of Surgeons.

[With two slight exceptions, previous to the year 1828, when Mr. James, of Exeter, introduced Chopart's operation into England, affections of the foot, whether from disease or accident, were abandoned indiscriminately to amputation of the upper third of the leg. Syme's operation, introduced in 1845, brilliant and scientific as it was, influenced the actual surgery of the foot but little.]

At this time British surgery boasted of but four standard operations connected with the foot—viz., excision of the astragalus, and Hey's, Chopart's, and Syme's amputations. And so matters continued until the year 1847, when Mr. Thos. Wakley performed his celebrated operation for resection of the lower ends of the tibia and fibula, with excision of the astragalus and os calcis. This was followed soon after by the equally celebrated operation of Mr. Teale, of Leeds, wherein he excised the os calcis, astragalus, and cuboid bones, and thus instituted a new era in the British surgery of the foot. To my mind, these two operations rank among the highest and most brilliant achievements of modern surgery.

Previous to this, as we have seen, operative surgery of the foot consisted in amputating between the disease or accident and the body. To remove a diseased bone from amongst healthy bones; to remove a bone from the posterior part of the foot without at the same time sacrificing the anterior portion of that organ, had been deemed (Liston's case excepted) an impossibility. But these operations showed that not only could these impossibilities be overcome, but that even disease, unpreceded by injury and situated in the posterior region of the foot, might be removed—nay, might even be removed with advantage to the patient; and that disease of this region, contrary to what had been previously imagined, did not of necessity involve the destruction of the entire foot. The result was what might have been expected; surgeons began to shake off the trammels of custom, and to think for themselves. They began to consider the beautiful mechanism of the foot, or any portion thereof, however small, as something worth preserving; and, accordingly, we now enumerate as standard operations, in addition to those already enumerated, "excision of the ankle-joint," "excision of the os calcis," "excision of the cuboid bone," "subastragaloid and Pirogoff's amputations," besides other operations performed in almost every region of the foot.

On the 1st of May, 1855, Mr. Teale surpassed his previous operation in removing from the foot of a young woman, aged twenty-two, the calcaneum, astragalus, navicular and cuboid bones. The patient returned in six months to her duties as a domestic servant, and is now engaged in making clothes by the sewing-machine. When she has much walking she relieves herself by the use of a stick. Mr. Teale has with the greatest kindness authorised me to present the cast of the case I now exhibit, in his name, to the museum of this College. In allusion to this and other operations he observes: "The general result has been very satisfactory. Indeed, I have adopted them as freely for disease limited to the posterior part of the foot as I have done Chopart's for disease limited to the anterior part. There is, however, one condition necessary to success—namely, that we select such cases as are the result of pure healthy (if I may so say) inflammation of ligaments, and not those which have originated in the cancellous structure of the bones, and which Sir B. Brodie rightly attributed, as I think (despite of what is now-a-days talked about as strumous synovitis), to struma."

A French author, in a treatise of comparatively recent date, asserts that no one has ever proposed, as a standard operation, to amputate through the continuity of the anterior row of tarsal bones. These operations, recommended by necessity, should never become the rule. Those surgeons who regard the foot as a solid whole, and who perform these operations, have the advantage of making a regular-shaped wound, but they expose the patient to the double danger of section of the bones and of the joints. Matthias Mayor, he continues, "in one of the eccentricities for which he was notorious, would reduce this study to its most simple form. According to him, the foot is but an undivided whole the same as the leg or thigh. The only questions which govern him are the extent or limits of the disease. He cuts through the soft parts and exposes the bone behind these limits, and applies the saw on their continuity as far as possible from the tibio-tarsal joint, without considering the small bones or the difficult joints which the instrument may divide. We will not," he continues, "pause to refute this doctrine of the surgeon of Lausanne, which, if he will pardon the expression, would rather cut the Gordian knot than try to untie it." I cannot help thinking that the eccentricities of M. Mayor, at all events in this instance, take a very sensible form, and that he is a better surgeon than his criticiser; for, whilst *he* acted from practical experience, the latter has evidently written from theoretical imagination only. Malgaigne, on the contrary (at page 277 of the seventh edition of his Manual), says, if by sawing the bones of the tarsus across, instead of disarticulating them, we can thus preserve a portion of the second row of tarsal bones, the result would certainly warrant our taking the method into serious consideration.

Having given this subject more than average attention for many years past, I entirely endorse M. Mayor's views. I would reduce this study to its most simple form. I would desire to be governed by the tangible reality of the extent or limits of the disease rather than by the doubts and uncertainties of abstract theories, and I consequently advocate strongly as a standard operation "to amputate through the continuity of the first or second row of tarsal bones in preference to sacrificing joints when there is no necessity for such sacrifice." I do this, not because I regard the foot as a mere undivided whole like the thigh or leg, but because I regard it as an aggregation of several parts, each perfect in itself, placed there for a given purpose; each and all of far too great value to be destroyed without a cause, and, above all, because the experience of many years has proved to me the safety and value of this proceeding.

Mr. Morgan, whose declining health has deprived the Bristol Hospital of his valuable services, appears, with the exception of Mr. Gay and Mr. Tudor (of Dorchester) to be the only British surgeon who, as far as I can discover, has systematically followed this plan. He writes me—"I may perhaps be permitted to state the result of my experience on two points of practical interest. 1st. In amputation of the anterior part of the foot, more forward than Chopart's operation, the patients do very well by ignoring the joints and sawing through the bones at the point of election." Mr. Gay also says—"I always saw through the row of tarsal bones, keeping as close to the front articular processes as possible; for in a case which I had in the Free Hospital in 1845, the wound was kept open by the extrusion of the cartilages." Whilst Mr. Tudor observes—"I never found the slightest disadvantage in sawing straight through the bones, whatever part of the foot it may be in, without following the principles laid down in some books of passing the knife between the articulations."

I advocate this practice also because I regard the joints themselves as of especial value as completing the mechanism of the parts connected with them. The medio-tarsal joint, for instance, completes the posterior segment of the longitudinal arch. Destroy this joint, and the calcis and astragalus at once become deprived of their anterior support, with the consequent elasticity and motion. Can anything be more unphilosophical than to advocate the sacrifice of any bone or joint of the foot for no other reason than that a particular operation should be performed? But yet this has been done, and in the following terms:—"The scaphoid bone is sometimes left in Chopart's operation in front of the astragalus through inadvertence. We once saw Dupuytren commit this error. Lisfranc even relates a case in which he saw the scaphoid left, and which ended successfully." It is scarcely possible to conceive greater ignorance of the subject or of the value of the parts than these observations evince.

The result of my experience convinces me likewise that the danger of pyæmia from cutting through the tarsal bones has been greatly exaggerated; indeed, it would seem after all to be of very rare occurrence in this country, for judging from the returns with which I have been favoured from all parts of England of the several operations performed upon the foot, the occurrence of this malady does not amount to one per cent. upon the number of operations performed. And indeed when we compare the extent of cut surface exposed in the soft parts with that of the bones, it is difficult to understand why, when it does occur, it should be attributed so entirely to the bone and not to the soft parts; and I have moreover seen it so frequently follow operations where no bony structure was either exposed or involved, that I must confess my scepticism in the matter. And to me it is an interesting fact, as supporting this opinion, that Mr. Syme, in enumerating the advantages of his operation, observes, "The tibia and fibula are divided through the *concellated* structure, and you thus avoid the danger connected with opening the medullary hollow, especially where there is a disposition to phlebitis."

[Disease in the bones of the foot is frequently so superficial that the removal of a very thin slice is sufficient to eradicate the mischief. Disease commencing in an articular surface may exist there for a considerable period without invading the cancellated structure of the lower end of the tibia. Mere increased vascularity and softening without actual change of structure or deposit of tuberculous matter is not sufficient to justify the condemnation of the entire bone.]

I would venture to offer the following suggestions as to operations on the foot:—

That we should perform our operation as close to the diseased or damaged structure, and preserve as much of the foot, as we possibly can do with safety to our patient.

That where practicable, we should cut through the tarsal bones with a saw in preference to disarticulating them.

That we should avoid the destruction of joints whenever we can do so.

That disease of one articulating surface does not of necessity demand the removal of the entire bone ; as, for instance, when confined to the tarso-metatarsal joints, or to the joints between the cuneiform and scaphoid, it is not always necessary to remove the whole of the cuneiform bones on the one hand, or of the entire scaphoid bone on the other. In such cases, however, the diseased portion should be removed by a clean cut made with a saw, and not bruised off by a gouge or chisel.

That whilst the openings in the skin cannot be relied upon as indicating the exact situation of the bone mischief, the existence of these openings, even if there be several, or the thickening and discoloration of the skin and soft parts, do not of themselves contraindicate operative procedure, since, as pointed out by Sir W. Fergusson, the soft parts, when relieved of the source of irritation, will speedily return to their natural condition.

After partial amputation of the foot, the bones, ligaments, and muscles, especially in young patients, appear to accommodate themselves to the altered condition of parts. The bones will, in some instances, become changed even in size and form, whilst the muscles contract new adhesions. These, however, are not the only phenomena which occur, tending to the reparation of parts. I allude to the deposition of new bone, either from the extremities of long bones, or from periosteum, or from the cut short bones.

Textor expressly asserts the new formation of bone, and this to a precise extent necessary to produce a new and perfect articulation of the ends of bones upon each other. He states that he found lengthening of the ulna to the extent of a quarter of an inch on which the radius moved, as in the natural condition, and the trochlea of the humerus appeared as perfect as if none of it had been taken away. Wagner, on the other hand, asserts that examination during life of persons who have recovered after extirpation of short bones, shows that the defect has been remedied, as far as possible, by the approximation of of the bones which lie nearest together, the interval which is left appearing to be filled up with a ligamentous material, in which, according to Reid, masses of cartilage or bone are sometimes deposited.

Sir W. Fergusson, in a case which he amputated at the tibio-tarsal joint seven years after he had extirpated the inner half of the os calcis for caries, found the bone partly regenerated, and the place of the lost substance partly occupied by a fibro-cartilaginous material.

I would also remind you of the following highly practical observations made by Mr. Hilton whilst your Professor of Surgery:—"Surgeons should be careful not to disturb unnecessarily the granulations or periosteal membranes which they meet with in operations associated with diseased bone, but on the other hand, leave all the vascular granulations which surround the diseased bone and line the inner aspect of the periosteum; for experiments, experience, and sound physiology show that if we can secure these bone-producing vascular structures from local injury, we give Nature a fair chance of manifesting her powers of speedy reproduction or removal of bone. No doubt in this case [one which he had described] a representative cuboid bone has been reproduced, for there is an equal and continuous ossific resistance along the whole length of the outer side of the tarsal portion of the patient's foot."

We have, however, still much to learn with respect to the deposit of new bone, which we frequently observe takes place without either of the above-mentioned causes. The mere alteration in the relation of parts would seem to be sufficient for its production. Witness the result of an unreduced dislocation of the head of the humerus or of the femur; the displacement of a tendon from its natural groove (as seen in the preparation before you). The tendons of the peronei muscles were displaced from their groove behind the fibula; there was no fracture, no exposure of cancellated structure at that situation, but nevertheless we find a new osseous hook, or hemular process, thrown out for the purpose of compensating these tendons for their lost position. And again, observe what obtains when muscles are violently or accidentally separated from their attachments. I have seen three cases of violent detachment or rupture of the adductor longus muscle from the pubes. They all occurred in the hunting field, the patients having been forced to throw the adducter muscles into sudden and violent action from their horses shying, jumping, or lunging unexpectedly. In neither case did I see the patients for some weeks after the occurrence. They all informed me that they felt something snap at the time; that they experienced great pain, and could no longer grasp the saddle; that they had gradually lost the pain, but were now frightened by finding something form in the upper and inner part of the thigh. Upon examination I found the adductor longus muscle in all the three cases separated from its attachment to the pubes, in one instance to the extent of nearly two inches, whilst the interval between the two was filled by a bony styloid process, or exostosis, originating from the body of the pubes, and giving attachment by its other extremity to the detached muscle.

[Various modifications of Syme's operation have been proposed.]

Blandin and Handyside advocate lateral flaps, alleging that they prevent bruising and twisting of the soft parts in removing the os calcis; but, as Mr. South justly observes, there is no necessity to bruise or twist the soft parts, whilst the side flaps do away with one of the principal advantages of Syme's operation, the preservation of the dense structures especially designed for supporting the weight of the body.

Messrs. Spence and Goodsir commence by the usual dorsal incision, and then carry the knife deeply across the plantar surface of the heel, from the internal to the external malleolus, where the incisions commenced. Having thus formed the flaps, they cut through the external lateral ligament, and keeping the knife close to the bone, divide the internal lateral ligament and soft parts on the inner side of the foot, which is next turned forwards. The tendo Achillis is then cut through, and the bone easily turned out from the plantar flap. The benefits they assign are greater facility and rapidity, and less twisting and manipulation of the plantar flap.

The late Dr. R. J. Mackenzie, of the Edinburgh Infirmary, advised: "The point of the knife to be entered at the mesial line of posterior aspect of ankle on the level of the articulation, and carried obliquely downwards across the tendo Achillis towards the outer border of the plantar surface of the heel, along which it is continued in a semilunar direction. The incision then curved across the sole of the foot, terminates on the inner side of the tendon of the tibialis anticus, an inch in front of the inner malleolus. A second incision is carried across the outer side of the ankle in a semilunar direction between the extremities of the first incision, the convexity directed downwards, and half an inch below the external malleolus. The flap is next dissected up, the knife being kept close to the bone to preserve the whole thickness of the soft parts; and by holding the base of the flap between the finger and thumb as it is detached from the bones, all risk of wounding the artery is avoided. The foot is then severed at the articulation, and a thin slice, with the malleoli, removed by a saw, as in Syme's operation."

M. Sédillot advocates a similar proceeding, which is said to be superior to that of Syme in cases wherein the integuments on the outside of the foot are unsound; in old people, whose arteries are not well suited to bear with impunity the stretching which occasionally occurs in separating the integuments from the heel process; and lastly, from its being more easily performed.

Mr. Quain, on the other hand, maintains the plantar flap, but after making the cut across the sole of the foot from one malleolus to another, he carries a second at right angles with it on the outside of the foot back to the heel between the point of the outer malleolus and the margin of the foot. He cuts the inferior flap short, and bevels off the edges of both flaps, so that skin should meet skin. He first performed this operation in the year 1851; and claims as its advantages greater facility of performance, and diminished probability of collections of matter and occurrence of gangrene.

But of all the modifications, that which has attracted the greatest attention and taken the firmest hold upon the profession is the one introduced by Pirogoff during the Crimean war, and which is now universally recognised as Pirogoff's operation. This consists in sawing through the os calcis from above downwards in front of the heel process, and leaving the latter undisturbed in the soft parts constituting the posterior flap, instead of dissecting out the entire os calcis as in Syme's method.

Pirogoff's method, as given by Mr. Spencer Wells, is as follows:—"He commences his incision close to the front of the external malleolus, carries it vertically downwards to the sole of the foot, then transversly across the sole, and lastly obliquely upwards to the inner malleolus, where he terminates it a couple of lines anterior to that process; the inner extremity being carried some lines more forward than the outer, to avoid wounding the posterior tibial artery prior to its division. Thus all the soft parts are divided at once to the os calcis. He then connects the outer and inner extremity of the first by a second semilunar incision, the convexity of which looks forward, and cutting through the soft parts down to the bones, he proceeds to open the joint from the front, cuts through the lateral ligaments, and thus exarticulates the astragalus. He then places a small amputation-saw upon the os calcis behind the astragalus, exactly upon the sustentaculum tali, and cuts through the os calcis, so that the saw passes into the first incision through the soft parts. Lastly, he separates the short anterior flap from the two malleoli, and saws through both of them close to their bases. He then turns the posterior flap forwards, and bringing the cut surface of the os calcis in apposition with the articular surface of the tibia, he unites the two flaps by sutures, always leaving the angles of the wound open to admit of free discharge. Should the articular surface of the tibia be diseased, it is sometimes necessary to saw off a thin slice from it with the malleoli."

It would appear from this description that when the articular surface of the tibia is healthy, Pirogoff brings the cut surface of the os calcis in contact with the cartilage investing that articular surface. At all events it is doubtful whether he does or does not. I regret that I cannot clear this point up. I wrote to M. Pirogoff expressly upon this subject, but as yet I have not received a reply.

Be this as it may, he claims the following advantages for his operation:—

1st. The tendo Achillis is not divided, and so we avoid all the disadvantages connected with its injury.

2nd. It also follows that the base of the posterior flap is not thinner than its apex, whilst the skin on the base of the flap remains united with the fibrous sheath of the tendo Achillis.

3rd. The posterior flap is not cup-like as in Syme's method; and its form, therefore, is less favourable to a collection of pus.

4th. The leg after the operation appears an inch and a half (sometimes more) longer than in the three other operations (Syme's, Baudens's, and Roux's); because the remnant of the os calcis left in the flap, as it unites with the inferior extremities of the tibia and fibula, lengthens them by an inch and a half.

5th. It serves the patient as a point of support.

Up to April, 1853, he had performed the operation three times, upon patients aged respectively twelve, thirteen, and nineteen years. All recovered well, and two walked without crutch or stick, and without limping. He adds a remark well worthy of notice, and one also which confirms what I have already advanced in my second lecture: "That notwithstanding the suppuration and the considerable gravitation of pus in the third case; notwithstanding the softness and fatty degeneration of the os calcis, which could be cut with a knife, still the remnant of the os calcis united firmly with the tibia. In the second case (he adds) he found pus in the joint during the operation, the cartilages inflamed and decayed, the ends of the bones softened, and a fatty degeneration; yet the result was most successful." As regards this point, Mr. Croft, in his excellent paper "On the Experience of the Surgeons of the *Dreadnought*" in this operation, remarks: "I may observe that, although the os calcis may be diseased at and about the articulating surfaces in instances of scrofulous disease of joints of tarsus, it is rarely that the posterior part is rendered too unhealthy to be made use of in the formation of the stump."

Mr. Busk recommends that the os calcis should be cut obliquely from behind forwards, instead of vertically. And for this purpose he inserts the saw well behind the calcaneo-astragaloid articulation. He says that the advantages of his section of the bone are: that a more extensive surface of the bone is brought into contact with the tibia; that the remaining portion of the os calcis does not require to be rotated so much on its axis as would be required in Pirogoff's operation; that the tendo Achillis is not so much stretched; and that the portion of the heel naturally in contact with the ground still remains the basis of support.

Watson, of Glasgow, recommends the incision to be made across the sole of the foot from one malleolus to the other down to the os calcis. He then cuts through the os calcis obliquely to avoid the malleoli. Then resuming his knife, he places it between the cut surfaces of bone, and cuts the posterior flap upwards and backwards until he gets fairly behind the ankle. An assistant then turning this flap upwards, and at the same time gently drawing up the skin of the front of the ankle, Dr. Watson carries his knife round the front of the joint, dividing the tendons, and then with a saw completes the operation by cutting through the tibia and fibula downwards and backwards.

The originality of this proceeding has been denied by Dr. Martin, of Warrington, who writes that he saw Dr. Pirrie perform the same operation more than two years before; and, indeed, at p. 754 of Pirrie's work "On the Practice of Surgery," we find, *quoad* Syme's operation: "Instead of disarticulating the foot, and then sawing off the malleolar processes and thin piece of tibia, I have, after making clearance for the saw by sending the knife round the bones, sawn off the malleoli, &c., without effecting disarticulation. This shortens the proceeding; and, whenever I have performed this operation, nothing could have been more satisfactory than the results."

Staff-Surgeon Williamson, Fort Pitt, recommends that the lower ends of the tibia and fibula should be sawn off obliquely from before upwards and backwards. This facilitates the adaptation of the os calcis to the end of the bones greatly, and is certainly well worthy of consideration. He relates a case operated upon by him on Dec. 24th, 1858, wherein the os calcis fitted admirably; the wound healed readily, and a very solid stump resulted.

Mr. Curling adopted this method on Jan. 5th, 1859; but the result of his case is not given.

Sir William Fergusson also cuts through the os calcis from below upwards. He begins his incision at the internal malleolus, carrying it across the sole of the foot to the external malleolus. He then saws the os calcis through from below upwards, after which he makes the incision on the dorsum of the foot, and separates it from the leg. In one case he found the astragalus firmly anchylosed to the tibia. He therefore cut through tibia and fibula, and removed them altogether.

In treating of the advantages and disadvantages of Syme's operation, I considered that operation, as you may remember, upon its own merits, or at all events with reference to amputations of the leg only. I drew no comparison between it and Pirogoff's modification. I was anxious to avoid repetition; and, moreover, such comparison could only be made satisfactorily when we had the objects and *modus operandi* of both methods before us. As we are now in possession of these facts, I will proceed to consider the advantages and disadvantages of Pirogoff's operation as compared with those of Syme's, not only with reference to civil but also as regards military surgery.

Mr. Busk, who maintains Syme's originality in demonstrating that a useful flap could be made from the natural cushion of the heel, and who entertains the highest possible sense of the great merits of the operation, writes that since the introduction of Pirogoff's proceeding he has never had occasion to adopt it.

According to Mr. Busk, the advantages of Pirogoff's operation are: greater facility and rapidity of performance; less disturbance of parts to form the cushion; a solid instead of a hollow flap; and a greater length of stump by an inch and a half.

According to Mr. Erichsen, they consist in the stump being longer, and better adapted for pressure; in the readiness of the union betwen the two applied osseous surfaces; and in the less likelihood of the supply of blood to the posterior flap being interrupted, as its vascular communications are not much disturbed, and the plantar arteries can always be cut long.

I do not see the force of the latter proposition, as there is nothing in Syme's method to prevent the plantar arteries being cut as long as the posterior flap, which is all that can be done in Pirogoff's operation.

Mr. Paget informs me that he has performed Pirogoff's amputation several times with a completely satisfactory result, and that he thinks more highly of it than of any other operation of the kind.

Mr. Partridge considers that there cannot be a doubt of the superiority of Pirogoff's method over that of Syme.

Mr. Henry Smith, though, as we have seen, so successful in his performance of Syme's operation, says it is his intention to adopt Pirogoff's for the future, as of its superiority no one who has seen a few cases can doubt.

Mr. Wood's experience of Syme's and Pirogoff's operations has been favourable to both, but he thinks the stump of the latter operation has been generally better.

Mr. Cadge, of Norwich, has the highest opinion of both. He does not think there is much advantage in one over the other, as the resulting stump in both is to his mind the best possible.

The objections urged by Mr. Syme are—

First, that the os calcis is a bone predisposed to caries, and it is therefore undesirable to leave any part of it. Surgeon Cusack and Surgeon Little both performed *à la Pirogoff*. In both cases disease returned in the portion of the os calcis left behind, and necessitated the removal of the limb. The operation is certainly open to this objection, and undoubtedly cases similar to those here quoted sometimes occur.

For instance in a case operated upon by Sir W. Fergusson, he four months afterwards gouged away dead bone, and four months after that there were still dead bone and fistulæ.

In another case operated upon by the same distinguished surgeon, three months after the operation fistulous openings remained, though the patient could bear pressure.

Mr. Simon, in one of his cases, had to remove secondarily two pieces of dead bone, and at the end of 426 days union was still infirm.

In a second case he had to remove a portion of the os calcis. Obstinate sinuses remained, and the patient was not cured for 483 days.

We should not forget, however, what has been stated by Pirogoff—that, notwithstanding the os calcis has been so soft that he could cut it with a knife, still the remnant of the os calcis united firmly with the tibia, and the patient was able to walk freely without the aid either of crutch or stick.

Of fifty-eight operations performed by British surgeons, I find that, including the above two quoted by Syme, five were succeeded by amputation of the leg, of which one was performed a few days after the original operation for secondary hemorrhage.

Mr. Butcher, of Dublin, and Mr. Walsh, of Worcester, each relate a case in which they commenced their proceedings for Pirogoff's operation, but in consequence of the condition of the calcis found it necessary to convert them into Syme's operations. Mr. Lowe, of Lynn, in 1860, commenced Pirogoff's operation on a boy aged twelve, for caries of the tarsal bones. He found the os calcis diseased, and gouged it away, merely leaving the periosteum. Unhealthy ulceration, however, attacked the stump, and he was subsequently obliged to amputate the limb. Whether these gentlemen were unduly alarmed at the condition of the os calcis or not, these examples do not appear to me to detract from the value of Pirogoff's operation in proper cases. They add, however, greatly to that of Syme's, since they show the applicability of the latter where the former cannot be trusted.

Mr. Syme's second objection, that the operation is greatly complicated by the employment of chain saws, &c., is negatived by the fact that chain saws are neither necessary, nor are they at present employed in its performance.

His third objection is, that union between the two osseous surfaces is not likely to take place easily.

Mr. Busk has shown that this objection is groundless. In Mr.

Tudor's last case the bones had united on the twelfth day. In Busk's first case the patient stood on the fourteenth day. Mr. Croft says, in two of the *Dreadnought* cases, the calcis was firmly united to the tibia in three weeks; whilst the recovery of Mr. Partridge's two cases is stated to have been very rapid.

Mr. Syme's last and principal objection is, that the integuments of the heel, which should form a cushion for the patient to rest upon, are necessarily carried forward, and the end of the stump is covered by skin, which is not designed for sustaining the weight of the body.

If this objection were valid, it would at once reduce the operation to the level of Baudens' proceeding; but Mr. Busk has proved that when the os calcis is cut as he advises, that portion of the heel naturally in contact with the ground still remains the basis of support.

The great disadvantage attending this operation is undoubtedly the collection of matter in the sheaths of the tendons so frequently observed after its employment. Pirogoff attributes this to the tendons being cut too short, and retracting either within or beyond their sheaths. He adds that he fears nothing so much; and strongly advises that the tendons be fixed before and during the operation, and that the limb be maintained in one position, as the best means of avoiding this complication. Mr. Busk, on the other hand, reports that suppuration occurred in some if not all of his cases, notwithstanding the methodical application of compresses, as suggested by Pirogoff; whilst as we shall see further on, Messrs. Marsh and Ashdown, of Northampton, purposely cut the tendons as short as possible, and both their cases escaped this complication.

Mr. Tudor, of Dorchester, whom we have seen, as one of the surgeons of the *Dreadnought*, among the first to perform this operation in England, says: "It appears to me that Syme's is in many cases to be preferred, because we know that when one tarsal bone is affected the others very soon become so, and this from constitutional causes; and though at the time of operation the os calcis may not be diseased, still from the extra pressure put upon it, and from the abnormal position in which it is placed, I should fear risk was incurred by exciting that disease in this now *more* important bone, which we may suppose originally existed. The comparative length of the stump I consider to be of little moment, because the difference is so very trifling. Again: I consider the amount of skill required in the performance of Pirogoff's operation to be greater than in Syme's," &c.

Mr. Henry Thompson objects that, in consequence of the increased length of stump, it is more difficult to adjust an artificial foot. Neither can he imagine that the retaining a portion of bone in the lower flap can contribute in any way to a rapid convalescence.

I have collected, as far as I have been able, the particulars of 58 cases of Pirogoff's operation performed by British surgeons. I have not in this table included those reported by Pirogoff; for although this eminent surgeon reckons nearly 100 cases performed in Russia, he does not give any available statistics to enable us to arrive at a satisfactory or definite conclusion. Of the above 58 cases, suppuration in the course of the sheaths of the tendons and in the neighbourhood of the stump is especially noticed in 11; whilst sloughing of the flap occurred only in one case, and in that it is attributed to disease of the blood vessels. 5 cases out of the 58 terminated fatally, or about $9\frac{9}{10}$ per cent. Of these 1 died of pyæmia, 1 of exhaustion attending kidney-disease, and three after accidents—2 on the same evening, and 1 in four days. Five suffered secondary amputation. Of these, the original operation in 4 had been performed for caries, and in 1 for accident, the secondary operation being required for hemorrhage. 43 terminated favourably, affording the patients good and useful stumps. Of these 25 were performed for caries, 2 for frost-bite, and 18 for accidents. The result of 3 is not known.

With regard to the period of cure I am enabled to give you a somewhat higher per-centage of results than I could of Syme's; for whereas of 219 cases of the latter I could only give the results of 40 (rather less than one-fifth), of 58 cases of Pirogoff's I am enabled to give the result of 15 (a fraction above one-fourth).

Of these 15 cases, 8 had been operated upon for caries, and 7 for accident.

Of the 8 former—

1	recovered with firm stump	in	7	weeks.
1	,, ,,	,,	8	,,
2	,, ,,	,,	12	,,
1	had firm stump, but also fistulous openings ...		12	,,
1	recovered with firm stump		40	,,
1	,, ,,	,,	69	,,
1	had infirm stump		61	,,

Of the 7 latter—

1	recovered with firm stump	in	6	weeks.
1	,, ,,	,,	7	,,
1	,, ,,	,,	8	,,
1	,, ,,	,,	10	,,
2	,, ,,	,,	12	,,
1	,, ,,	,,	24	,,

So that whilst in the former class of cases the period of recovery varied from 7 weeks to 69, in the latter it varied from 6 to 24 weeks.

Croft reports that in 3 cases the remnant of the os calcis united to the tibia in 3 weeks; in 1, in 12 days.

Mr. Busk reports that one of his patients stood on the 14th day. He saw another 7 months after the operation; he had in the meantime been a voyage, and worked as a seaman.

Mr. Sympson, of Lincoln, states that a patient upon whom he performed this operation can now walk fourteen miles a-day without fatigue.

Mr. Partridge also reports the recovery of two of his cases as being very rapid.

Mr. H. W. Budd, of Worcester, has, with the greatest kindness, furnished me with the particulars of the two following original cases:—

J. C——, aged twenty-six, admitted into the Worcester Infirmary on Feb. 24th, 1866. Foot smashed by a railway accident. The astragalus, os calcis, with the integuments of the heel, uninjured. A wound extended from about two inches above and behind internal malleolus, across the front of the ankle-joint, to a corresponding point above the malleolus externus. The integuments of the upper margin of the wound were separated upwards for an inch. They were cold, and there was extravasated blood here and there. Although sloughing was apprehended, Pirogoff's operation was decided upon. The case went on favourably. The patient has an excellent stump, half an inch shorter than the sound one.

March 31st. Union of bone perfectly firm. He has been walking about well for some weeks. A very small portion of integument sloughed.

April 29. Discharged cured.

J. L——, aged eighteen, admitted, under Mr. Budd's care, Feb. 26th, 1866, with disease of ankle-joint and tarsus of twelve months' duration. There were two sinuses, one on each malleolus, communicating with dead bone. Great pain on pressing astragalus against tibia. Considerable swelling of foot.

March 26th. Pirogoff's operation performed. All the bones of ankle-joint were diseased. The os calcis was very soft, and the soft parts were infiltrated with gelatinous or scrofulous substance. This infiltration and the os calcis compelled Mr. Budd to remove more of this bone and the tibia.

The case did well, without any bad symptom. The stump, Mr. Budd says, is a famous one, the *best* he ever saw, one inch and a-half shorter than the other leg.

Discharged cured May 20th.

Mr. Ashdown, of Northampton, has also kindly supplied me with the particulars of the two following cases, which certainly go far to establish the value of Pirogoff's amputation :—

E. P——, aged twenty-nine, married, a very delicate woman, four months advanced in pregnancy, admitted May 16th, 1858. Three hours previously a gun, charged with common-sized shot, fell from its support, and went off on the ground a few feet from her, the contents striking the sole of her left foot. There was copious hemorrhage at the time. When admitted, after a journey of twelve miles, she was collapsed ; pulse quick and feeble ; countenance pale and anxious. When the bandages were removed there was considerable oozing of venous blood. The integuments and muscles of the sole of the foot, the heel excepted, were ploughed up from the bones ; the metatarsal bones all more or less fractured and comminuted ; the integuments generally livid and pulpy, and the great toe hanging loose from the rest of the foot.

The patient having rallied sufficiently in a few hours, Mr. Marsh removed the foot by Pirogoff's amputation. In so doing the posterior artery was divided above its bifurcation and ligatured, and Mr. Marsh having made the tendons protrude as much as he could, cut them short off, and brought the parts together. For the first two days the patient was very feeble, feverish, and sick, the vomiting (of pregnancy apparently), which had been troublesome for above a month, being now constant. She ultimately, however, did well, and six weeks after the operation was out of doors, and in another week left the hospital. She was in due course confined, and was ill for a long time after, but for many months past has walked upon the stump regularly, and without inconvenience.

E. B——, aged forty-two, admitted under the care of Mr. Ashdown, Feb. 24th, 1860, having had her feet entangled in the machinery of a thrashing machine a few hours previously. Mr. Ashdown performed Pirogoff's amputation on the right foot. The wound healed by first intention, and the patient did well until the tenth day, when bilious vomiting came on, and lasted a week. She, however, ultimately did well.

My old friend and pupil, Mr. Folker, of Hanley, has not only kindly furnished me with the particulars of the following case, but also with the cast upon the table, taken from the patient's stump :—

T. H——, aged twenty-one, had his foot severely crushed by an engine and tender running over it on the 8th of November, 1865. Mr. Folker at once performed Pirogoff's amputation, removing about three-quarters of an inch of tibia and fibula. A small abscess formed, but speedily healed after being opened. In a month the patient was allowed to get up, and could bear some weight on the stump. He left the infirmary on the 16th of December, able to walk without pain, the bones being firmly united. He is now following his former occupation of a coach-trimmer, and walks backwards and forwards to his work every day. He uses a walking-stick, but can do very well without it.

Mr. Henry Smith has also kindly supplied me with the following very interesting case :—

An artilleryman, aged twenty-six, had extensive disease of the right ankle-joint, for which Mr. Smith performed Pirogoff's amputation on the 8th of August, 1863. A remarkably rapid recovery took place, and in seven weeks the patient was well. Mr Smith saw the man a year afterwards : the stump was well formed, not at all sensitive, and he could walk well by the help of a simple bucket apparatus applied to the leg.

My colleague, Mr. Canton, also performed Pirogoff's amputation in the case of a young lady, and with excellent results.

Dr. George Buchanan, of Glasgow, has likewise published a case in which he successfully performed Pirogoff's amputation upon a man whose foot had been crushed by machinery.

Mr. Shepherd, of Worcester, kindly informs me that he performed the operation upon a railway-guard on the 29th of October, 1864, for compound fracture of the tarsal bones, the os calcis alone remaining uninjured. Secondary hemorrhage occurred, and rendered amputation of the leg necessary. The man, however, recovered.

[Velpeau, in 1839, says in the second volume of his "Operative Surgery" that M. de Lignerolles had communicated to him a method of operating at the ankle-joint, whereby instead of the malleolar projections, a nearly flat broad surface is left. Leaving the astragalus the os calcis must be removed with the foot. The flaps must be made from the sides of the foot, and be reflected towards the malleoli before the foot is disarticulated. In 1846 Malgaigne described his method, which is exactly that indicated by Lignerolles.]

He performed his first operation in 1845, and the following is his plan of proceeding, taken from the seventh edition of his Manual :—He obtains his flap from the inner part of the sole of the foot. Placing his knife over the tendo Achillis, he cuts through skin, tendon, and fat, down to the bone or upper surface of the os calcis ; he then continues the incision along the outside of the foot just below the external malleolus, turns it up over the dorsum of the foot a little in front of the medio-tarsal joint, then over the inner border of the foot, and across the sole of the foot for the inner two-thirds of its breadth. By this incision he divides everything down to the bone. Then, transferring his knife to the inner extremity of the first incision (over the tendo Achillis), he cuts obliquely downwards, at an angle of forty-five degrees, on to the sole of the foot until he joins the other incision, thus completing his flap. This he reflects first from the sole of the foot, taking care to include all the soft parts except some of the tendons nearest the bone; next from the inner side and dorsum of the foot, until the articulations between the calcis and astragalus, and between the latter and the scaphoid, are exposed. This flap being held back by an assistant, he proceeds to open the medio-tarsal articulation, or that between the astragalus and the scaphoid, and at the same time divides the external calcaneo-astragaloid ligament; and introducing his knife horizontally beneath the head of the astragalus and between this bone and the os calcis, he cuts backwards, following the direction of the anterior calcaneo-astragaloid articulating surface, through everything he meets with, until, the division of the anterior fibres of the interosseous ligament allowing the bones to be separated, the operation is completed without further difficulty.

He admits that the flap may be made in front from the skin covering the dorsum of the foot, or behind from the integuments covering the heel, as in the tibio-tarsal amputation; but he prefers the mode of proceeding just related.

It will be observed that, with the exception of one flap being made in this method instead of two, the operation here described in no way differs from that suggested by Lignerolles, whose name curiously enough, is entirely omitted from the paper. Nor is the slightest allusion made to Textor's operation or to the announcement by Velpeau; although, as we have already seen, the former was performed in 1841, whilst the latter was published in the year 1839—seven years before Malgaigne submitted his proceeding to the Academy of Sciences.

Subastragaloid amputation, although an excellent proceeding in proper cases, has not been warmly supported in France; whilst in England it scarcely appears to be known, if I may judge from the replies to my inquiries. Malgaigne says that he has performed it six times; and that exclusive of these M. Vacquez has collected eleven other observations, including one performed by Nélaton. In England, as far as I know, it has, up to this time, been performed only by Mr. Simon, Dr. John Traill, of Arbroath, Mr. Garner, of Stoke-upon-Trent, and by myself. As the operation is in reality a very good one, and one which in proper cases may be adopted with great advantage to the patient in preference to either Syme's or Pirogoff's, we can only attribute this neglect to the fact of its not being sufficiently known. Its advantages may be briefly stated, that whereas both in Syme's and Pirogoff's proceedings the ankle-joint is of necessity sacrificed, in subastragaloid amputation, not only the ankle-joint, but at the same time an increased length of limb is preserved.

True it is we are told that Hyrtl, from purely anatomical considerations, rejects this operation altogether, and it would seem that Pirogoff endorses his views; but he does not inform us in what these anatomical considerations consist; and I must confess my inability to do so myself, for I am not acquainted with a single fact connected either with anatomy, physiology, or pathology, which, in judiciously selected cases, would militate against it if properly performed.

In advocating this proceeding, however, I do not for one moment pretend that it will or ought to supersede Syme's or Pirogoff's methods altogether. They, I believe, will always maintain their ground. They can be performed when this cannot. But I am equally confident that the two former have frequently been selected in instances wherein the latter might have been performed with greater advantage to the patient; and I also feel certain that, when this operation of subastragaloid amputation becomes more generally known, surgeons will study their cases more closely, that they will pay greater attention to the selection of their operative proceedings, and that it will be more frequently adopted. Mr. Syme justly claims an increased length of limb and a stump formed of structures especially supplied by nature to sustain pressure and to enable man to walk thereon. Pirogoff and his supporters claim superiority for his proceeding over that of Syme in that his (Pirogoff's) operation is more easily performed, that by it a still greater length of limb is obtained, and that whilst the same soft structures enter into the stump, a new osteo-plastic measure or principle is introduced in obtaining union between the cut surface of one bone with another, the other being a different bone from that with which nature originally designed it to articulate. For subastragaloid amputation I claim equal facility of execution with that of Pirogoff, and length of limb equal to that afforded by the latter; but whereas in the latter this increased length of limb is obtained by a piece of bone transplanted, so to speak, to new ground, to an abnormal situation, the former supplies it by a non-disturbance of parts, by the preservation, in fact, of the astragalus in its natural position. Another advantage consists in the dorsal incision being made over the medio-tarsal joint, or a little anterior to it, instead of being carried in front of the ankle-joint. By this the sheaths of the extensor tendons are left undisturbed, and consequently the risk of inflammation and suppuration within those sheaths and in the course of the tendons is naturally diminished.

But the great superiority of this operation over those of both Syme and Pirogoff is that by it the ankle joint is preserved intact, whilst a stump of natural heel-structure is equally obtained.

A surgical writer of considerable reputation, in treating of subastragaloid amputation, ignores Malgaigne's mode, and advises the heel-flap to be made as in Syme's. He adds : "A good, long, and useful stump results ; but the number of cases requiring it must be small, as it does not often happen that the calcis, with the anterior range of tarsal-bones, are diseased without the astragalus also being involved."

Mr. Syme objects to the operation altogether ; "that Dr. Traill, of Arbroath, lately sent a communication, with cast, to the Medical Society of Edinburgh. He says truly that cases in which the astragalus could be left must very rarely occur, since disease affecting the tarsus at all, generally involves the astragalus ; but I (Mr. Syme) think it would be very undesirable to leave this bone, even when it is not affected at the time of the operation, on account of the predisposition to caries, one of the great recommendations of amputation at the ankle being that it removes all the bones liable to disease."

Nothing, it must be allowed, can be more sweeping, nothing more condemnatory of the poor tarsal bones, than are these opinions. The first asserts that it does not often happen that the calcis and the anterior range of tarsal bones are diseased without the astragalus being involved also. The second lays down that when disease affects the tarsus at all it generally involves the astragalus. Whilst the third maintains, never mind whether the astragalus be sound or not at the time of operation ; it is predisposed to disease, and, therefore, should always be removed.

These opinions are so extreme that I must be excused for doubting whether their authors, when they promulgated them, had duly considered what the practical result would be if they were implicitly followed. That they have influenced the profession, and that they continue to do so, all who read the recorded surgery of the foot must admit, and consequently if erroneous the sooner their influence is dissipated the better. They are either of great value or they are actually mischievous. They negative all efforts at improvements in the surgery of the foot ; they condemn resections of bones : they condemn partial amputations ; in a word, they condemn all operative procedures, save that of removal of the foot at the ankle-joint. They virtually ordain that if disease appears in a tarsal bone, never mind how limited that disease may be, you must amputate at the ankle-joint at once. It is no use your removing an isolated bone ; it is no use attempting to preserve any portion of the foot ; the tarsal bones, whether diseased or not, are predisposed to disease, and therefore the sooner they are all sacrificed the better.

If these opinions are so strongly opposed to the preservation of bones not yet implicated, how decidedly fatal must they be to the proceeding of gouging away, or attempting to gouge away, the portions of bones in which disease has already manifested itself; for if the remaining bones not yet implicated are so predisposed to mischief that they ought of necessity to be removed, it is sheer folly to imagine that gouging away a diseased portion of bone can have the effect of converting the remainder into such a perfectly sound condition as to emancipate it from this predisposition. Yet we find the following case reported by the high authority to whom I have alluded :—

"The ankle-joint appeared sound; the articulations on the inner side of the foot were sound; there were several fistulous openings on the outer side of the foot, and the probe found the outer side of the os calcis diseased. Another opening led to the diseased astragalus, and another to the cuboid. The disease appeared to be limited to the upper and anterior part of the os calcis, the anterior and outer portion of the astragalus, and the posterior of the cuboid. *The parts were gouged away*, and in so doing the large anterior synovial membrane between the scaphoid and cuneiform was laid open. The day after severe erysipelas set in, followed by suppuration in the sole and inner part of the foot, and the patient became so weak that amputation of the leg was obliged to be had recourse to."

Now as I have not this dread of "predisposition," as I believe that caries of the tarsal bones frequently results from disease originating in the soft structures of the tarsal joints, and as frequently extends to the bones from mere contiguity of surface, and, moreover, that the removal without violence, of the carious portion of a bone will frequently arrest the disease in that bone altogether, I should say that, circumstances contraindicating Teale's operation, the case just related was one to which subastragaloid amputation was especially adapted, when performed according to the method which I shall presently describe.

I would submit, moreover, that the operation may also be performed with great advantage in such cases as the following related by Mr. Skey. In Nov. 1858 he removed a considerable amount of diseased tarsal bones from the right foot of a patient. The case went on well for a month, when the disease returned, and in May, 1859, Mr. Skey operated again. In his clinical observations, alluding to the proportion of failures in these attempts (one in every three), he says it might have been preferable to have amputated, but that he desired to give the man a chance. As the ankle-joint does not appear to have been implicated, I should in similar cases prefer subastragaloid amputation in the first instance to gouging; and in the event of the latter having been tried and failed, I would certainly select the former rather than repeat the gouging or have recourse to amputation of the leg.

In one case, instead of taking the flap from the sole of the foot, Malgaigne obtained it from the dorsum. The patient, a woman, aged forty-two, had for three years suffered from disease extending from the scaphoid to the os calcis, but the ankle-joint remained sound. Gouging was had recourse to in the first instance, but the patient getting worse, Malgaigne operated by a dorsal flap. This sloughed away, but the wound ultimately cicatrized. The patient, however, subsequently complained that the weight of the body bore too much on the head of the astragalus, which in fact descended lower than the rest of the bone. By degrees she got over this, and when she left the hospital in April, 1846, she could walk comfortably.

Maisonneuve appears to have performed the third subastragaloid amputation in France. He first cut the skin across at the posterior part of the heel; then bringing his knife forwards beneath the external malleolus, cut from behind forwards along the outer border of the foot to the roots of the toes, so as to make a flap involving the skin of the entire dorsum of the foot. The flap united, but the patient subsequently suffered greatly in walking, from the pressure on the head of the astragalus, which descended lower than the malleoli, the posterior part of the bone being apparently drawn up by the contraction of the tendo Achillis.

M. Malgaigne also relates the case of a soldier operated upon before Sebastopol, in whom the astragalus, drawn up behind by the tendo Achillis, became anchylosed to the tibia, and thus prevented the stump resting flatly on the ground.

In the year 1848, Mr. Simon introduced the operation into this country in the case of a boy aged fifteen, suffering from scrofulous caries affecting the front and outside of the tarsus, extending to the os calcis, but sparing the astragalus. Having ascertained that the astragalus and ankle-joint were healthy, Mr. Simon made his flaps as in Syme's amputation and, introducing his knife between the astragalus and calcis by cutting from before backwards, detached the foot. The boy recovered with a good and useful stump; and, when seen several years afterwards, could walk well and without inconvenience.

Mr. Simon's second case was not so successful, although the fatal termination can scarcely be ascribed to the operation, performed on a railway labourer, aged twenty-five years, admitted into St. Thomas's Hospital in the summer of 1853. A carriage-wheel had passed obliquely over his foot from behind forwards and inwards, crushing everything in front of the line of the incision, and breaking and exposing the under part of the os calcis. The same amputation was performed as in the former case. The stump was nearly healed, and was apparently as satisfactory as could be wished, when about a fortnight after the operation, the patient died of tetanus.

Both Malgaigne and Maisonneuve allowed the head of the astragalus to remain; and we are led to infer that Mr. Simon pursued the same practice. In the two former instances we have seen that great suffering and inconvenience follow this practice, resulting from the tendo Achillis forming fresh attachments, and drawing the posterior portion of the astragalus up into the mortise of the tibia and fibula, by which the head of the bone, rendered unduly prominent, was forced downwards; so that the patients in walking rested on this process instead of upon the broad under surface of the bone, as they should have done. This inconvenience may be entirely prevented by removing the head of the astragalus at the time of operation, which can be done with perfect safety, and certainly a much better stump results from such proceeding.

On the 24th September, 1863, the girl a portion of whose foot I had amputated by sawing through the cuneiform and cuboid bones, and whose case I alluded to in my second lecture, was readmitted with inflammation and pain in her other (the right) foot. The right ankle-joint appeared to be much swollen and inflamed, especially over its anterior and outer aspect. Entire rest upon a splint was ordered. The joint, however, still remained swollen, thickened, and painful. and at length the skin on the outer side ulcerated. Under tonics and nourishing diet, with perfect rest, the patient appeared to improve; but at the expiration of six months she became rapidly worse; several wounds opened in the neighbourhood of the joint, instep, and beneath the sole of the foot, with sinuses leading to the bones in various directions. As her general health was now giving way, I, in March, 1864, recommended the girl to have the foot removed at the ankle-joint; and she having consented, I proceeded to perform that operation. But a more careful examination under chloroform led me to doubt the necessity of sacrificing the ankle-joint. I consequently made an exploratory incision on the outside of the foot, and with my finger found that the astragalus was sound, whilst the freedom with which the foot could now be flexed and extended showed also that the ankle-joint was free from implication. Under these circumstances, with the consent of my colleagues, I determined upon performing the subastragaloid amputation by flaps as in Syme's operation, differing only in making the posterior flap fuller than that advised in the latter proceeding. In consequence of the amount of disease in the os calcis the soft parts had become detached, so that very little difficulty was experienced in separating the posterior flap from the bone, or in subsequently introducing the knife between the astragalus and calcis and cutting the interosseous ligament. Having thus detached the whole of the foot with the exception of the astragalus, I next removed the head of the astragalus, sawing through the neck of the bone, and bringing the flaps together, united them by two or three sutures, taking care to leave the angles unclosed so as to admit of free discharge. The stump healed gradually and without a single untoward symptom. The patient's health slowly but surely improved, and at the end of three months she was discharged, being able to bear her weight upon her stump and hobble about upon crutches. After a few months Mr. Ernst, the surgical mechanician, fitted her with artificial feet; and she is here in the college to-day for the purpose of being examined, if you, sir, and the rest of my hearers would desire to do so. You will find that both the stumps are entirely healed, and sound. It is now more than two years since the last operation, and nearly three since the first. Notwithstanding the amount of disease in the tarso-metatarsal joints, and the proximity to those joints at which I sawed through the tarsal bones, as you have already seen in the preparation before you, the stump has remained sound to the present day, and in the condition in which you will now find it. The stump resulting from the subastragaloid amputation appears to me to be perfect. It is round and of good form; the cicatrix is firm and well up in front; and the bottom of the stump is properly covered by the natural heel-tissue. She has a certain amount of motion of the ankle-joint, and can now walk with the assistance of a single crutch. Not, as she will tell you, that she requires the crutch from inability to bear her weight upon the stumps, but because, as she says, the loss of the front of her feet causes such a strain upon the lumbar muscles when she walks without the crutch that she is prevented doing so by the pain referred to that situation.

Subastragaloid amputation appears to have been performed up to this time in twenty-two cases. By M. Malgaigne six times, by M. Maisonneuve once, by M. Nélaton once. M. Vacquez has collected nine other cases, all of which are stated to have terminated favourably. In Scotland, Dr. John Traill, of Arbroath, performed it once successfully. In England, Mr. John Simon has performed it twice, once successfully, and in the other case the patient died at the end of a fortnight of tetanus. Mr. Garner, of Stoke-upon-Trent, operated in one case, but he does not report favourably. And, lastly, I performed it once, in 1864, successfully.

Thus we have three amputations connected with the ankle-joint: first, Syme's, removing all the bones of the foot, furnishing an excellent stump, but the shortest limb; Pirogoff's, removing all the bones of the foot except the tuberosity of the os calcis, also supplying an excellent stump, and a limb as much longer as the quantity of os calcis preserved; and, lastly, the subastragaloid amputation, by which the astragalus and ankle-joint are saved, and an equally good stump and length of limb are bestowed on the patient as by Pirogoff's method. Good, however, as the subastragaloid amputation may be, and satisfactory as have been the results in the case just related, it has seemed to me to be capable of improvement in two essential points—namely, first, in the length of the limb, and, secondly, in the amount of leverage afforded by the astragalus for the artificial foot, and the consequent effects upon the movements of the ankle-joint. It therefore struck me that cases might occur in which, whilst the astragalus remained sound, disease or accident involving the other tarsal bones might only extend to the anterior portion of the os calcis; as in the first of the cases of Mr. H. Budd, of Worcester, in which the astragalus and os calcis alone remained uninjured, and the sound integuments were too scant to admit of a Chopart's amputation; and that in such cases a modification of subastragaloid and Pirogoff's amputation might be beneficially introduced to the notice of the profession. I proposed, in fact, to leave the ankle-joint intact, to remove the head and under surface of the astragalus, and, removing the whole of the os calcis with the exception of its posterior third, to bring that portion and adapt it by its cut surface to the under surface of the astragalus. On mentioning my views to my colleague, Mr. Canton, he not only entered into them warmly, but, with that courtesy for which he is proverbial, offered me the first case which he might have to which the proceeding could be applied. He kept his promise, and by his great generosity I am now enabled to present you, not merely with a theoretical description, but with a living proof of the success of an operation which has never, I believe, been performed before, either in this or in any other country.

The patient, aged twenty-four, a native of Ayrshire, about twenty years ago, during the winter, observed that his great toe became swollen and inflamed. He did not attach much importance to this at the time, as others of his family had their feet and toes similarly affected, caused, as it was supposed, by frost; therefore no especial notice was taken of his condition until the others had got better. After continuing some time in this swollen state, matter formed, which discharged by three openings, all. as it were, connected interiorly. It next suppurated at three places on the back of the foot, extending in a straight line from the great toe as far as the instep. Under treatment, however, these parts gradually healed up, and have remained so. For a considerable time, it might be two or three years, the disease was confined to the toe, and the peculiar excrescence was formed and gradually increased, but gave him little pain, so that he could go to school, or do what he was able about a farm of ninety acres which his father tenanted, with slight inconvenience, although there was always more or less discharge. The next suppuration was towards the outside of the foot and near to the toes: it gradually formed into an excrescence similar to that on the toe, but was removed by means of caustic, after which it quite healed up. By-and-by, however, new wounds were formed round where the others had been, which remained until the operation. About twelve years ago the disease in the sole of the foot commenced, and since that period there have been several abscesses on the leg at different times and in various places, all of which, after festering, soon got well under treatment, and there has been no disease save that in the foot for several years. Until the last two years the disease had not materially interfered with his walking, but since then it has become much more troublesome from an acute pain in the region of the heel and ankle. He has always lived in the country, and enjoyed pretty good health except when the matter was forming. He is the youngest of six sons, and has two sisters younger than himself, all healthy and strong. His father died at the age of seventy-six, and his mother is still alive, and the relations of both are strong and healthy people. In the winter of 1864-5 the little finger of the hand on the same side as the diseased foot became similarly affected, and has remained so ever since.

When admitted into Charing-cross Hospital the patient presented a well-marked example of the fair variety of scrofula. The foot was generally swollen and somewhat distorted. The soft parts have a brawny hardness. The great toe is so enlarged and misshapen as to appear elephantoid. In front of the ankle-joint, and particularly on the dorsum of the foot, there are many larger or smaller ulcerated openings, which give issue to a strumous form of pus, without, however, such odour as would indicate the existence of diseased bones, nor does examination with a probe show that these latter are affected. All the joints seem to be perfect, and it gave him no pain or inconvenience when they were moved.

No plan of treatment, either local or general, having ever been of any avail, it was decided that the disease should be removed by operation; and, as I have before said, Mr. Canton most kindly allowed me to carry out my proposition, assisted by himself and my two other colleagues, Messrs. Hird and Barwell.

The patient having been placed under the influence of chloroform by Mr. Marshall, I commenced an incision beneath and at the posterior angle of the external malleolus, and carried it forwards along the outer border of the foot to a point about half an inch anterior to the projecting base of the fifth metatarsal bone. I then made a second incision along the inner border of the foot, commencing posteriorly about the centre and beneath the internal malleolus, and terminating anteriorly at a spot corresponding to the termination of the external incision, and then united the two by a third and semilunar incision, carried, with its convexity directed towards the toes, across the front of the sole of the foot well down to the bones. Reflecting this flap back as far as the projections on the under surface, and in front of the tuberosity of the os calcis, I carried a fourth incision across the dorsum of the foot, immediately behind the head of the astragalus. Then, applying a saw upon the under surface of the os calcis as far back as I could, I cut through the bone obliquely from below upwards and backwards. Next, resuming my knife, I entered the mediotarsal articulation, and, passing the instrument under the head of the astragalus, and cutting from before backwards, in the direction of the anterior articulating surface on the os calcis, divided the interosseous ligament, and detached the front of the foot. I then sawed off the head of the astragalus, and with a pair of bone cutters (curved on the flat) removed the two articular cartilages on the under surface of this bone, and securing the two plantar arteries, thus completed the operation. The flaps were brought together by three wire sutures in front, the lower angles of the wound being left open. The patient has gone on remarkably well ever since. There has been no suppuration in the course of the tendons, no secondary hemorrhage, nor any constitutional disturbance. A very few days after the operation he said he was much more comfortable than he was before it was performed. As I believe the operation to be original, I have ventured to designate it in the table before you as my own.—*Lancet, June* 23 *and* 30, *Aug.* 4, *Sept.* 1 *and* 8, 1866, *pp.* 678, 705, 115, 227, 257.

29.—ON FRACTURES OF THE TIBIA AND FIBULA.

[The following article is from a report on the treatment of fractures in the London Hospitals, published in the Medical Times and Gazette.]

FRACTURES OF THE TIBIA.

Fracture of the Head of the Tibia into the Knee-joint. — This, unassociated with fracture of the condyles of the femur or compound fracture into the joint, is a comparatively rare accident. The treatment generally adopted for it is simply that of laying the limb out straight on a back splint and raising it, so that the muscles acting on the joint may be relaxed. In order to keep the joint at rest, gutta-percha, leather, or pasteboard splints may be applied to the sides of the knee. The limb should then be swung. Passive motion may be commenced in about a month or six weeks.

Fractures of the Shaft of the Tibia.—In cases of fractured shaft where the displacement is, if any, slight, after the fragments have been accurately adjusted, the limb is put up immediately by some surgeons in plaster of Paris or starch bandages, the methods of applying which we have already described in a former paper. This early putting up of fractures in the stiff apparatus should only be had recourse to when, as we have before observed, the displacement is slight and the integuments are not severely bruised

With regard to the splints and contrivances in use for putting up the more complicated forms of fracture, we can only say that we have seen those of the most simple possible construction; these, and the methods of fixing them to the limb, we now proceed to describe. We mention them here particularly with the tibia, as it is in fractures of that bone that they are most frequently used; but it will be seen that the same methods apply equally well to fractures of the fibula, or of both bones together.

In cases, then, of fractured shaft, when the fragments have been placed in apposition (which they may be judged to be, when the ball of the great toe is in a line with the inner border of the patella), a very common method of putting the fracture up is that of applying two side splints with foot pieces to either side of the limb. These, having been of course previously well padded, may be kept in position by taking a turn or two of bandage, first round the foot and ankle, and then round the foot-piece. The bandage should not be continued the whole way up the limb, as it is of importance in all cases of fracture that the actual seat of injury should be exposed to view; a few turns round the upper part of the leg-pieces will be sufficient to retain the splints in position. The limb may either be swung or placed between sand-bags; if the latter, a wire cage should be put over it to keep off the pressure of the bed clothes. These splints may be either hollowed out or plain. Sometimes only a single side splint with a foot-piece is applied to the outer side of the limb, the leg being slightly flexed, and placed on its side.

McIntyre's iron splint, or Liston's modification of it, is much used in some hospitals. Care should be taken in applying the splint, that it be thoroughly well padded throughout, and that cotton-wool or tow be inserted between it and any of the soft parts that may be subjected to pressure. No part of the splint should press upon the heel, as this part of the foot frequently becomes ulcerated from the carelessness with which this splint is applied. In applying it, after the foot and ankle have been carefully bandaged to the foot-piece, and the thigh to the thigh-piece, it is not necessary to cover the limb entirely with bandage. The apparatus should be swung either from a common cradle or Salter's swing. Mr. Erichsen says, "In some cases of fracture of the bones of the leg, however, McIntyre's apparatus is not applicable. This is more particularly the case when the fracture is very oblique, from above downwards and from before backwards. Under these circumstances, the fragments cannot be got into good position, so long as the limb is kept extended and resting on its posterior surface, the bones riding considerably, and one or other of the fractured ends pressing upon the skin in such a way as often to threaten ulceration." In some hospitals, in addition to the two splints above mentioned, a back splint, with a foot-piece (capable of being fixed in the required position by a screw) corresponding with the sole of the foot, and with the leg-piece extending as far up as the ham, is used. The splints may be retained in position either by bandage or fillets.

At St. Bartholomew's, Mr. Paget uses the following method: —The limb is placed upon an ordinary iron back splint; to this it is secured at the foot and knee by means of wide pieces of gutta-percha moulded to the shape of the limb, and laced under the splint by narrow tape. Two straight wooden side splints are then applied; these are buckled together at their lower extremity (which is exactly on a level with the sole) by means of a thin leather strap passing across the sole from the one to other. In the remainder of their length, which reaches up the thigh to the same point as the back splint, they are fastened more or less tightly, according to the degree of lateral pressure which it is thought advisable to apply to the limb, by two or three webbing straps and buckles. The leg is then "swung" in the usual manner. It will be seen that this way of putting up fractures of the leg recommends itself by its simplicity, and, if the term may be allowed, by its conciseness. No bandage at all is employed, and the limb is left wholly free from all pressure that can interfere with the circulation. The exact condition of the limb, and the relative position of the fragments can at any time be ascertained, and as the side splints are fixed independently of the back splint, they can be removed either during occasional sponging of the limb, or for any other purpose.

In cases of oblique fracture, with projection of the lower end of the upper fragment, the management of which is generally very difficult, Mr. Hutchinson finds that the simple expedient of raising the leg obviates this trouble.

Fracture of the Inner Malleolus.—Side splints, or a single splint applied on the opposite side to the fracture.

FRACTURES OF THE FIBULA.

Fracture in the Upper Part.—The displacement here generally is so slight that plaster of Paris or dextrine bandages may be used at once, or the limb may be placed in McIntyre's or side splints.

Fracture of the Lower End, or Pott's Fracture—the foot being dislocated outwards, and the tip of the inner malleolus broken off, or the internal lateral ligament ruptured.—The most general method is to put the limb up on a Dupuytren's splint, which is like a short Liston's splint with a couple of notches at the lower end, through which the bandages pass to produce slight extension.

In applying this apparatus, which is adapted to the inner side of the leg, care must be taken to pad the splint well, especially opposite to the seat of fracture, which padding should fill up the hollow formed by the curve of the leg as it lies on the splint, and so act as a fulcrum over which extension can be made. A few turns of bandage should then be made round the foot and through the notches. The bandages should not be continued up higher than the ankle, and the upper part should be bound securely to the leg just above the calf. Sometimes an ordinary side splint with a foot-piece is used, but it should be carefully padded in the way mentioned above, as the object to be attained is the throwing out of the upper end of the lower fragment. After union the dextrine or plaster of Paris bandage should be applied.

Fractures of both Bones.—Any of the above-mentioned splints are in general use. A very simple method we saw in use at the *Dreadnought* Hospital ship, in cases of fracture of both bones. It is very easily applied, and can be readily taken down without disturbing the setting of the fracture. A piece of stout sheeting is to be rolled in opposite directions, so that two pillows or junks are formed by the rolled extremities; upon this the patient's limb is laid. Then a fillet is passed behind the sheeting, and similar ones under the limb. Next two well padded side splints, with foot-pieces, are adapted to either side of the leg, and secured by buckling the fillet passing under the leg. Next the external sheeting is rolled up in opposite directions, until the rollers correspond with the outside of the splint, and the fillet under it buckled over all. The foot may be secured to the first piece by a turn of bandage. Hester's apparatus we have seen very generally used in the Westminster Hospital. In Mr. Heath's valuable work "On Minor Surgery" is the following account of the manner of applying it. He says, "It combines the advantages of the side splint with those of the swing, as the splint is suspended from rollers and a cradle, which allow of the free movements of the patient. In making use of it, the knee is first firmly fixed, a pad being placed below the tubercle of the tibia, and another above the head of the fibula, the foot being well strapped to the footboard. Extension is then made by means of a screw underneath, and thus shortening is effectually prevented, whilst the side splints, when screwed on, keep the limb perfectly steady."

We lately saw, under the care of Mr. Hutchinson, in the London Hospital, a child, about 4 years old, who had, in falling, broken both bones of the leg, and we were told that a second case of the same kind had a short time previously been under treatment. The limb was put up in splints, and "swung" in the usual way. This accident is so rare that it deserves to be mentioned. Every one must have observed that while in adults fractures of the bones of the leg are much more common than fractures of the femur, in children just the reverse is the fact. Fractures of the femur are very common, indeed much more so than in the adult, while fractures of the leg are very rare. We have often asked for an explanation of these facts, but have not yet met with one that is altogether satisfactory. It seems probable that they depend to some extent on the different way in which children and adults fall, the former generally coming to the ground with the legs doubled under, so that the weight of the body falls, through the knees, on the femur, while adults more commonly sustain fracture while the weight of the trunk is being transmitted to the ground through the bones of the leg. —*Medical Times and Gazette*, *June* 23, 1866, *p.* 662.

30.—ON THE OPERATION OF TREPHINING IN CASES OF FRACTURE OF THE SPINE.

By Dr. Robert M'Donnell, F.R.S., Surgeon to Jervis-street Hospital; Lecturer on Surgery in the Carmichael School of Medicine; Examiner in Surgery in the Queen's University.

[The operation of trephining the spine in any case whatever has practically been set aside for many years past, but the writer of this paper knows that there are many who coincide with him in thinking that the operation should be acknowledged in certain cases to be justifiable. Dr. M'Donnell has now seen the operation performed three times on patients, and he has performed it many times on the dead subject.]

In the lumbar region and lowest part of the dorsal the operation offers the greatest difficulties. The patient should be placed face downwards, with the feet towards the window, so that the light may fall into the bottom of the wound. The body should be placed as evenly and flat as possible on its front aspect; but the person who gives chloroform can keep the head turned a little on one side, and somewhat raised, by placing one of his hands under the chin. An incision from four to five, or six, inches long is to be made through the integuments over the spines, with a strong scalpel. A strong curved bistoury is the best instrument with which to divide the muscles on each side of the spinous processes; and this can be done, at once, more rapidly, and closer to the bone, by entering the point of the bistoury below and cutting upwards and backwards. As soon as the mass of muscles on each side are to a sufficient extent divided, the operator should fix, himself, on each side, a retractor, and give each in charge to an assistant. The broad flange of such a retractor not only keeps the wound well open, so that the operator can see what he is about, but by pressure prevents hemorrhage, while the hand of the assistant who holds it does not get fatigued, owing to the other end being bent. The operator should next feel in the sulcus, on each side of the spinous process, for the inequality caused by the injury, and by grasping successively each spinous process, in a pair of necrosis forceps, ascertain whether the posterior portions of any vertebra are broken. He must then determine which laminæ are first to be divided; and before doing this it is well to place sponges, wet with iced water, in the hollows on each side of the spinous processes, and give time for all hemorrhage to cease.

Having singled out the spinous process of the vertebra, the laminæ of which are to be divided, the interspinous ligaments above and below it are to be cut, and a part of the process taken off, with bone forceps, leaving, however, enough to grasp with the necrosis forceps, when it becomes necessary to remove that portion, after division of the laminæ. The division of the laminæ, in this region, is exceedingly difficult. The trephine cannot be used; and Hey's saw, owing to the depth of the wound, and oblique manner in which the bone must be sawn, is also useless. The division is best accomplished by strong forceps bent at an angle, something like the forceps known as "Fergusson's side-cutting forceps." Experience shows that the laminæ may be thus cut through without fear of injuring the cord or its membranes. As soon as this has been accomplished, the portion of the spinous process still remaining is to be caught in a pair of necrosis forceps, and the portion of bone raised and removed by carefully dividing the ligamentous structures still holding it. When the posterior arch of one vertebra has been taken away, the removal of the second is comparatively easy; the best method of effecting this is by nibbling it away bit by bit, with the ordinary gouge forceps.

It is a much easier matter to remove a spinous process, and the laminæ on each side of it, in the middle or upper part of the dorsal region than in the loins. In its preliminary stages the operation is like the foregoing. When the muscles on each have been divided and retracted, as already described, owing to the mode in which the spinous processes of the dorsal vertebræ are imbricated one over the other, it becomes necessary to remove the spine above that which, with its laminæ, is to be first taken away. This having been done the spinous process of the vertebra, the laminæ of which are to be divided, should be taken away, as close to the root as possible, with gouge forceps. With a little care the spinous process can be so completely taken off as to leave the surface sufficiently flat for the application of the crown of a trephine, which should in general be about one inch in diameter, and furnished with a strong centre pin, a little longer than usual. As the bone is of a much softer texture than that of the cranium, the trephine once planted, works rapidly, so that after a few turns the centre pin should be withdrawn, and it should then be worked gently until it is found possible to move, and subsequently elevate, the trephined portion; after this is taken out it will be seen that in the dorsal region there is a sort of provision which enables one to use the trephine without much danger of its, in any way, injuring the cord. In fact the articulating process of the vertebra, next below, looks backwards in such a way as to prevent the instrument from going suddenly in so as to injure the medulla. The posterior arch of a second vertebra may now be easily taken away, either by a second application of the trephine, or better still, by the aid, simply, of the gouge forceps.

In the cervical region the removal of the bone can be effected with great facility by the gouge forceps alone; here, as well as in operating on the first dorsal vertebra, the trephine cannot safely be employed. After the skin has been divided and the muscles detached on each side the interspinous ligaments are to be cautiously cut, taking care not to sink the scalpel too deeply; the spinous process is next taken off with the gouge forceps, and at once the ligamentum subflavum comes into view, and the vertebral canal is opened to a small extent. Bit by bit the posterior arch is then to be taken away by the gouge forceps, until enough is removed. The removal of a second or third can be even more readily accomplished with the same instrument. The retractors employed for holding back the muscles on the neck should not be so broad as those intended for the operation on the back or loins, and the lower edge of the retracting flange should be somewhat convex.

In all the cases which I have seen the wounds have gone on favourably under the simplest treatment; water dressing at first; and after suppuration has become established, charpie, with simple dressing, or a poultice with some "warm dressing" under it.

It is unnecessary to say that every possible attention must be paid to the general health of the patient. He should be placed on a water bed, the bowels and bladder carefully attended to, and any tendency to bed-sores guarded against with the utmost anxiety. For this purpose he must be shifted from side to side, and occasionally placed upon his back, even at the risk of pain in the wound or disturbance of the vertebra. To avoid bed-sores and keep the bladder in good order must be the first object of the surgeon, and no entreaties or complaints should influence him in deferring to do what he considers necessary for this purpose. The bed clothes must be frequently changed, and kept perfectly dry. If any bed-sores have already formed, every effort should be made to get them cured; if the urine is already alkaline and muco-purulent the bladder should be washed out every day, or several times each day.

Immediately after the operation the object of medical treatment should be to avoid inflammation of the cord or its membranes, and with this view belladonna or atropia should be administered, with the intention of keeping the medulla as much as possible in a condition of repose; opium should not be given; but if urgently demanded to allay pain it should be joined with belladonna, in doses proportionally large. Somewhat later iodide of potassium may be ordered, and when all apprehension of inflammation is past, strychnine. Indeed, it will be in different cases a very nice and at the same time very important question to determine whether it may not be good practice to give strychnine early, even at the risk of calling forth inflammatory action in the cord. Of course, its administration would not be contemplated if any tendency to myelitis can be detected. But if pressure on the medulla for a longer or shorter time has given rise to that kind of mechanical softening of it which, without being identical with white or atrophic softening, is closely akin to it, and if it seems that the parts below are not recovering their vital powers after the pressure has been got rid of, it may become necessary to make a desperate effort to rouse the dormant energies of this portion of the marrow, regarding the chance of exciting inflammation of it as the less great danger of the two. Under these circumstances I should not hesitate to give strychnine cautiously before many days after the operation.

Shampooing of the limbs and the use of electricity should be early applied in order to arrest the atrophy of the muscles. Whatever doubt may exist as to giving strychnine at an early period after the operation there need be none about its administration after the risk of inflammation is past; it should then be added to good nourishing diet, with wine, beer, and iron tonics.—*Dublin Quarterly Journal, Aug.* 1866, *p.* 31.

31.—ON THE REDUCTION OF DISLOCATIONS, MORE ESPECIALLY OF THE HIP AND SHOULDER, BY MANIPULATION.

By THOMAS NUNNELEY, Esq., Leeds.

[Hitherto, reduction of dislocations of the hip-joint by manipulation has never been attempted except after failure of the ordinary methods of treatment. Even Dr. Reid, of Rochester, United States, to whom is owing the honour of first introducing reduction by manipulation as a legitimate plan, only speaks of reducing dislocations on the dorsum ilii and into the ischiatic notch. Mr. Nunneley believes manipulation to be as applicable not only to the two other forms of dislocation to which the hip joint is liable, but equally so to all those to which the shoulder joint is subject. Mr. Nunneley continues:]

The most important condition to be insured is a *relaxed, but not perfectly helpless, flaccid, uncontractile condition of the muscles;* as it is by the contraction of the muscles, which are attached near to the head of the dislocated bone, that reduction is mainly accomplished. The importance of constantly bearing this in mind I cannot too strongly impress upon those who may feel inclined to attempt the plan of reduction by manipulation. If the muscles, which are more immediately concerned, be in a state of active, rigid contraction, as they commonly are after a dislocation has existed for a short space of time, and the patient has recovered from the immediate shock of the accident, they will almost inevitably prevent those movements of the bone which are essential for its being brought into a position for slipping into the socket, whence it has been removed; while, on the other hand, if they be incapable of any contraction whatever, it will frequently be found to be impossible for any manipulatory movements of the surgeon to replace the bone; or, being replaced, for its being retained in its position. I feel confident that I have seen both of these causes materially interfere with success, particularly the latter one, when the muscles have been entirely paralysed, owing to the anæsthesia having been rendered too profound. In the wish to prevent any muscular resistance whatever, the necessity for some contractility has been forgotten, or not understood. If the patient be still suffering from the shock of the injury, sickness, drunkenness, or, from age or any other cause, be in a depressed condition, he will hardly require to be put under chloroform or any other anæsthetic ; but, on the contrary, if the patient be young and vigorous, if the muscles are in strong contractile action, or have been shortened by the loss of their normal opposing resistance, particularly where the dislocation is of long standing, or where there are adhesions to overcome, then the anæsthesia should at first be sufficiently profound to allow of free manipulatory movements bēing effected, so as to have overcome all such resistance by the time returning contractile power in the adjuvant muscles may be ready to pull the bone into the socket.

As the first commencement of manipulation, particularly in cases of recent date, the movements made with the limb should be very gentle, not rough nor abrupt. The limb should be well flexed : then, when all the muscles are relaxed as much as possible, and, so to speak, thrown off their guard, the limb should be suddenly and rapidly, but not violently, rotated on its axis, and completely abducted or adducted, as the case requires, and at the same time depressed or raised, when the adjuvant muscles will be perceived to rapidly contract. Thus the bone is partly pushed by the surgeon, and partly pulled by the muscles attached near to its neck into its place. A snap is commonly, but not invariably, heard ; and the reduction is accomplished. This, I believe, can only be rapidly and brilliantly accomplished under the muscular conditions which I have indicated.

The sound heard, and the impression made upon the surgeon's sense of touch, on the slipping of the head of the bone into the socket, are so peculiar, that I do not think he will often be deceived as to whether the reduction has been effected or not, after he has once successfully accomplished it.

I need hardly detain the members by any very minute description of the precise manner of proceeding in each form of dislocation, as this must necessarily be varied to some extent with each form of dislocation, and must be guided by the surgeon's own knowledge of the anatomy of the joint ; the object being, I would merely say, to use the limb as a lever to bring its displaced head into as near a position to its socket as is possible ; at the same time calling into action those muscles which will immediately act upon it in pulling it through the ruptured capsular ligament into the socket, and at the same time relaxing those which may have a contrary effect.

As a rule, I think the patient should be laid on his back upon a mattress placed upon the bed, the floor, or a large table, so as to be firmly supported. When the muscles are thought to be sufficiently relaxed, and the patient is quite quiescent, the surgeon, supposing the dislocation to be on the dorsum ilii, or into the ischiatic notch, should mount upon the mattress, and, standing above him (if it be a dislocation of the shoulder, standing beside the patient on the affected side will be the proper position), quietly seize, with both hands, the leg of the dislocated limb; gently flex it upon the body; adduct it; then quickly rotate the head of the bone, which brings the muscles into play; and, almost at the same instant, suddenly and forcibly abduct the limb and bring it down into an extended position, when, if the movements be successful, the reduction is effected. If the head of the bone be in the ischiatic notch, while the movements will be of essentially the same character, the flexion will be required to be somewhat less than when it is high upon the dorsum; while the abduction, as the limb is rotated, must be more forcible, or the head of the femur will not be lifted out of the notch over the high edge of the acetabulum; and care, as has already been said, must be taken, or, instead of passing into the cavity, it will be thrown into the thyroid foramen. Should the dislocation, whether primary or secondary, be on this foramen, the movement must be one of full adduction, instead of abduction, so as to bring the head of the bone near to the socket.

Though this plan of reducing dislocations has been spoken of as one more especially adapted for cases in which the head of the bone is in the ischiatic notch, I believe it will be found that, in this position, reduction by manipulation will be more difficult, and more frequently fail, than in any other form of dislocation to which the hip is liable.

I may just add that, of anæsthetics, I prefer the Dutch liquid (chloride of olefiant gas), or the bromide of ethyle, to chloroform; as, amongst other advantages, they both seem to me to have decidedly less tendency to induce muscular contraction and that violent struggling which chloroform so commonly induces before the system is brought fully under its influence, and which it is of importance to avoid, as the more quiescent the muscles are the more easily and readily will reduction be accomplished.—*British Medical Journal, Oct.* 20, 1866, *p.* 434.

32.—ON DISLOCATION AT THE SHOULDER JOINT.

By Dr. G. Hamilton, Falkirk.

[The difficulty in reducing a dislocated shoulder is frequently that it is impossible to fix the scapula whilst extension is made. Many plans have been recommended and tried, such as putting the arm through a common ladder, putting a transverse bar in the axilla, and placing the patient in a high-backed chair with his arm extended over the back.]

About two years since, I met with rather a difficult case, in the person of a large-bodied and very muscular man, in which I took advantage of a huge arm-chair, with a strong high back, which I found in the house. On this I placed a pillow, for the axilla to rest upon, and with the assistance of two strong men I reduced the dislocation very satisfactorily. Another followed, shortly afterwards, where I had no suitable arm-chair, but where I found a common screen for drying clothes, and this, with the pillow, also did very well. In a third case, neither of these being at hand, I mounted the patient on a table, placed the axilla on a pillow on the top of a door, and succeeded equally well. About six months since, I had, unfortunately, to make personal acquaintance with this accident. In passing over a railway bridge, my horse took fright at a passing train, and came down with me. In stretching out my right arm to save myself, dislocation at the shoulder took place, of which I was immediately made aware by the ugly tearing sensation that occurred. Fortunately, a house was near at hand, in which I received shelter. Without losing a moment, I looked about for some suitable apparatus with which to effect reduction. Finding nothing better, I got a narrow table, on which I placed, on its side, a long narrow stool, such as is found in cottars' houses. On the top of this I had a pillow placed, on which I rested my axilla, my body being placed between the two feet of the stool. Two strong men, who were at hand, kindly lending their assistance, reduction was effected after a few minutes' traction. I was so much pleased with the results in these instances, that I was thinking of having constructed a suitable apparatus which I could keep by me for use in such dislocations, when I cast my eyes upon a set of painters' steps, which immediately struck me as precisely the article I wanted. I have used this now in three cases, and its use seems to me to give very considerable advantages over the modes of reduction generally employed.

The "steps" I use are 4 feet 10 inches high, and the moveable support should be fixed with an iron rod, and not with a rope, as is often the case, as the former secures a greater amount of steadiness. A pillow is laid across the top step, and the patient ascends as high as may be convenient, of course placing the axilla on the top of the pillow. One or two assistants now lay hold of the arm, drawing, at first, steadily outwards and slightly downwards, traction in the latter direction being gradually and cautiously increased by approximating the arm to the steps. Reduction, in all the cases I have had, has been effected easily, and even, if I may use the expression, elegantly, but none of the dislocations had remained unreduced for more than twenty-four hours. The great power that we here possess, however, seems to me to render it highly probable that, in cases of longer standing, this simple apparatus will also be found very efficacious.

The three agencies mainly to be relied on in ordinary cases of shoulder-joint dislocation are evidently extension, counter-extension, and leverage, and especially the combination of these. When the dislocation has remained long enough unreduced for adhesions to form, perhaps, also, the putting in practice preliminarily some such manœuvre as Sir Astley Cooper saw the Lancashire bone-setters use, where they rapidly whirled round, the arm before attempting reduction, may be of importance to the operator.

In using the "steps," their height is very convenient for exercising extension, while the counter-extension required is made to a great extent by the weight of the patient's body, the rest being easily supplied by the foot of an assistant. The height, again, is very important in exercising leverage power, and its amount at command is enormous, and of course requires caution in its use. In laying hold of the arm of a person placed in position for experiment, I have the feeling that I could with ease, if I wished, produce either dislocation or fracture of the humerus. Here, also, the combination of these powers is easy and natural, simply by causing the assistants to approximate the arm to the steps. Almost all our best surgeons have dwelt upon the importance of employing leverage in these cases, and yet the usual modes of reduction supply this very inefficiently. The heel in the axilla, or the knee of an assistant, gives us but little; while, when the pulley is employed, leverage power, from the points of extension and counter-extension being fixed, is necessarily lost altogether. To remedy this, I recollect seeing Mr. Liston, as he recommends in his "Operative Surgery," endeavour, with a towel under the patient's arm, to *lift up* the head of the humerus; but the power given by this means is evidently very slight compared with such leverage as can be got in using the "steps." With these, even should the pulley be used, leverage could easily be combined with extension, by gently moving the steps forward; or, perhaps, this might be done more effectually and continuously by having wheels attached to the steps.

In brief, this modification of the usual modes of reduction of these dislocations, which I have proposed, seems to possess the advantages,—

1st. Of enabling the surgeon to dispense with his personal exertions.

2nd. It gives an amount of power in extension and leverage limited only by a consideration of the resistance possessed by the tissues; and it also enables the operator easily and naturally to combine these powers.

3rd. The position of the patient gives perfect freedom for the administration of anæsthetics, if such should be wished or required.—*Edinburgh Medical Journal, Sept.* 1866, *p.* 248.

33.—ON THE CURE OF CLUB-FOOT WITHOUT CUTTING TENDONS, AND ON CERTAIN NEW METHODS OF TREATING OTHER DEFORMITIES.

By Richard Barwell, Esq., Assistant-Surgeon Charing Cross Hospital.

[Mr. Barwell considers that tenotomy is at present used to an extent which is unreasonable in theory and unsatisfactory in its results; and, as patients shrink so much from being cut, it is our duty to carefully consider any mode of practice which professes to accomplish, without an operation, that which has hitherto required the use of the knife.]

If we look at an ordinary case of *talipes valgus*, or splay-foot, we can hardly fail to be struck with the appearance of weakness and relaxation which the whole ankle and foot present. There is evidently a want of proper force and tone in the tissues. Such being the case, it certainly seems unreasonable to divide the contracted tendons—which is in effect to paralyse those muscles which still retain their contractile power—and yet the free division of the tendons, and the application of a "Scarpa's shoe," is the treatment which is commonly recommended in these cases. Now, it is the soundness of such practice as this that Mr. Barwell calls in question.

It will be observed that we are speaking of those cases of club-foot which depend upon paralysis, and we apprehend that they form the largest class with which the orthopœdist has to deal. Assuming, then, that the common cause of the deformities which we are considering is paralysis—that the muscles have lost something of their contractile power—the free division of tendons, and the application of a firm, unyielding shoe, does not seem likely to restore the muscular force or to strengthen the ankle-joint. Moreover, the foot is a complex organ, composed of several distinct portions, which are articulated together; and it is undesirable to act upon it as a whole, in the way that the mechanical appliances so often do; but it should be treated as consisting of at least two separate parts. Following out this train of reasoning, Mr. Barwell has asked whether tenotomy and the application of a mechanical shoe is necessary at all, even in the severer cases of club-foot. Does not such treatment add to the mischief? Does it not intensify the disease, which is essentially one of debility? This question the author answers in the affirmative; and then he proceeds to explain the method of treatment which he has devised, and which consists in supplying the place of the paralysed muscles by means of elastic cords. By a light and simple contrivance these cords are fastened to points as near the origin and insertion of the paralysed muscles as possible, and the patient is encouraged to use his limb, while at the same time constitutional remedies are prescribed, and local means, such as friction, galvanism, &c., are employed. Such is, in brief, the new method of cure for club-foot which Mr. Barwell has proposed, and which he has practised with apparent success during the last few years. Certainly it commends itself to us as a rational and physiological plan of treatment. It proceeds upon the principle that our aim ought to be to strengthen weak parts, and not to weaken strong ones—a principle which is gaining ground every day in the practice of medicine and surgery.

But is this method of treatment applicable to all cases of club-foot? We can hardly suppose that it is. Even if the principle is found to hold good to the extent which its author anticipates, there will always, we imagine, remain a balance of cases which will require tenotomy and the use of a mechanical shoe. Congenital malformations, contractions following injuries, abscesses, &c., — these cases will never be amenable to Mr. Barwell's plan; although after division of the tendons the use of elastic cords has been found of great service. We should have been glad if our author had discriminated more carefully the cases to which he considers his method is most applicable. This is a point of great importance in recommending a new plan of treatment. Nothing can be more damaging to an experiment than that it should be tried upon an unsuitable subject. We would therefore advise Mr. Barwell to lay down clearly and distinctly the cases which he finds can be cured by his method, and to mention the classes of cases in which it is less applicable. At the present day, when the resources of our art are so numerous, the highest surgical skill consists in a wise discrimination of cases, and in the selection of the means of cure most suitable to the particular case in hand. Every one must have noticed how prone men are to fall into a routine of practice, and to deal with groups of disease rather than with individual cases.

In some of the severer cases with which he has dealt, Mr. Barwell has found it a good plan to begin the treatment by putting the patient under chloroform, and making rapid extension. By this means he has been enabled, after the lapse of a few days, to apply the elastic cords with greater advantage, and to expedite the cure. But we should be glad to learn whether our author finds himself able to deal with the severest class of paralytic cases by means of his method. Given an aggravated and long-standing case of club-foot from paralysis: are the elastic cords sufficient to effect a cure? None of the cases that are detailed in the volume before us give sufficient answer to this question; for they are either cases of children, or cases in which the disease had not long existed.

Following up his subject, Mr. Barwell has adapted his method to the treatment of knock-knees and crooked shins; and he has devised means by which spring force may be brought to bear upon these deformities in such a way as to draw the bones into a more natural position. An ingenious instrument for the cure of knock-knees is described and figured at page 210. It does not, however, appear to us to be so happily devised as our author's method of acting upon club-feet. It stands out so far from the leg that one would think a boy could hardly wear it at all; moreover, it is rather complicated, and must interfere with progression. Might not the same principle which has been applied to club-foot, namely, the principle of strengthening the weak side, be employed in treating knock-knees? Might not a broad india-rubber band be stretched down the inside of the leg, while the outside was, if necessary, supported by a light splint? Would not such a plan be more in harmony with the physiology and pathology of the case than the method here described?

When we were speaking of the unsatisfactory results which often seem to follow the division of tendons, we made an exception in favour of the *tendo-Achillis.* This tendon, by reason of its size and its isolated position, unites readily and with comparative certainty. There cannot, therefore, be the same objection to cutting it that there is to cutting the other tendons; and we believe it is in simple cases of *talipes equinus*, that the best results of tenotomy are seen. Speaking of this operation, Mr. Liston says in his "Practical Surgery,"—

"Twenty-five instances of division of the tendon have come under our care within the last six or eight months, and an almost uniform success has followed the practice pursued. In some a rapid cure was effected, and in all marked good effects followed. Even in the most unfavourable instances, when the deformity was both great and of long-standing, the application of proper apparatus within a week or two after the operation, produced a more decided improvement in the form of the limb in two or three months than usually follows the adaptation of mechanical means alone in twice as many years."

It would appear, therefore, from this testimony among others, that the subcutaneous division of the *tendo-Achillis* is quite satisfactory. It is the practice of dividing the secondary tendons about the ankle-joint which is open to a question.—*British and Foreign Medico-Chirurgical Review, July,* 1866, *p.* 23.

34.—ON THE TREATMENT OF ENLARGED BURSA OVER THE PATELLA.

By William S. Savory, Esq., F.R.S., Assistant-Surgeon to St. Bartholomew's Hospital.

[There is as yet no single method of treating enlarged bursa over the patella, which is recognised by surgeons as at once safe and sure. The milder plans of treatment, by blisters, mercurial ointment, and iodine, are both slow and uncertain, whilst extirpation and the seton are liable to be followed by mischievous and even dangerous consequences. Mr. Savory does not think that the following method of treatment has received the attention it deserves.]

The enlarged bursa is punctured with a lancet, or small knife, at its most prominent point ; all its contents are carefully expressed, and then it is subjected to firm pressure by a pad of lint, strapping, and bandage, so that its walls may be everywhere kept in contact, and its cavity obliterated. This practice, according to my observation, is followed by one of two results. Either the bursa is obliterated outright, and there is an end of the case in a few days, or, in spite of the pressure, the bursa tends to refill. In this latter event, the fluid is again evacuated through the same puncture, and the bursa is again compressed. Then it may be cured, or may partially fill again, but perseverance in this plan soon succeeds. It must be observed, however, that when the evacuation is repeated, the character of the fluid which is discharged is gradually altered. It becomes more puriform. Therefore after two, or perhaps three, evacuations, particularly if there be any tenderness, a poultice is substituted for the pressure until the bursa subsides. But even in these last-mentioned cases, in which the plan is followed by the least simple result, a cure is, as a rule, accomplished more quickly and satisfactorily than by other means.

This simple plan of treatment may be adopted, I believe, in almost every case. It cures most quickly when the walls of the cyst are thin, and can be readily pressed together. It is most likely to require repetition, and to be followed by some suppuration, when the walls of the bursa are thicker and firmer. In some extreme cases, where the walls are very thick and the cavity a mere slit, the bursa appearing as a solid tumour, perhaps extirpation may be preferable, but I think this operation should be reserved for extreme cases. When the bursa is inflamed, a few days' previous rest is advisable. In the smaller bursæ the fluid has usually the characters of ordinary synovia; in the larger and thinner ones it is often more or less bloodstained from previous bruising, but this, according to my experience, does not at all interfere with the efficacy of the plan of treatment I am advocating. There can be no doubt, that, as a rule, the cure is more certain and rapid when the limb is not used while the pressure is kept up, but, in the great majority of cases, one need not insist upon quietude. Indeed, amongst the poorer classes, this plan of treatment has the great advantage, in many cases, of not depriving them of a single day's work.

The chief points to be attended to in the management of these cases are—

To secure, after the puncture, by gentle and careful manipulation, the complete evacuation of the contents of the bursa.

To take care that the subsequent pressure is equable and efficient, so that no cavity can remain.

To keep up the pressure long enough to prevent any chance of subsequent refilling. In every case the pressure should be continued for at least a week after the cure seems complete. It is safer, in some cases, to keep it up for a period twice or thrice as long as this. Indeed there is no excuse for discontinuing it too soon, for, if properly applied, it produces no inconvenience.

This plan of treatment is, of course, applicable to other bursæ, whether normal or adventitious, when enlarged and troublesome, provided it can be carried out. But in most cases there is little chance of being able to keep up firm pressure; there is nothing against which the bursa can be steadily compressed. But for the treatment of the ganglions which sometimes appear in the neighbourhood of the wrist, either in front, or on the back of the hand, this treatment seems preferable to any other. It is simple, safe, almost painless, and when thoroughly carried out, it rarely fails.—*St. Bartholomew's Hospital Reports, Vol. II, p.* 80.

35.—EFFICACY OF PERSULPHATE OF IRON AS AN HÆMOSTATIC.

By Dr. W. A. Wetherby, of New York.

[On March 3, 1866, Dr. Wetherby with Dr. S. T. E. Beck, of New York, were suddenly called to a gentleman upon whom the operation of excision of the tonsils had been performed a few hours before by one of the most skilful surgeons of the city.]

The blood was flowing at a very rapid rate; and, judging from appearances, the patient, who had already shown signs of syncope, had lost some forty or fifty ounces. We procured a drachm of Monsel's salt as soon as possible, and with a moistened probang covered with the powder touched the bleeding parts.

The effect was almost instantaneous; but lest there might be a recurrence of the difficulty, we dissolved the remainder in water, to be used as a gargle. The further management of the case was, as usual, very simple, and not a drop of blood flowed from the parts after the first application.

Case 2.—This ease is more singular in many of its features, and may, perhaps, suggest a more extended use of this preparation than has heretofore been made.

During the night of April 25, 1865, I was called, perhaps more to fulfil the requirements of decency than from any confidence in the efficacy of my services, to visit Dr. L. N., of this city, who was suffering from an alarming attack of hæmotysis, which had failed to yield to the more usual remedies, as lead, opium, &c. It had been preceded for several weeks by a severe cough, profuse purulent expectoration, extreme emaciation, and, in fine, by all the symptoms incident to that slow but sure destroyer, consumption. The patient himself was aware of his condition, and had lost all hope of recovery, but begged me at least to make an effort to prolong his life for a few hours that he might dictate some last bequests to his friends abroad. Emboldened by my previous success with the artlcle, and reflecting that all the usual legitimate preparations had failed, I sent for some *persulphate of iron*, very dry, and reduced to an impalpable powder. A small quantity of this was administered by *insufflation* into the lungs every hour during the remainder of the night and the following day. The success in arresting the hemorrhage was *perfect*, and encouraged by this, I was induced to experiment still further in correcting the discharge of matter from the lungs. Directing its use in the same manner, though with less frequency, for some time, and assisted by the action of general remedies, I soon had the satisfaction of seeing my patient leave his bed, his room, and in a short time, his house; and now, in a few months afterwards, he is prosecuting a large and successful practice in his profession, subjecting himself to all its arduous duties with no inconvenience or ill health.—*American Journal of Medical Sciences, July* 1866, *p.* 106.

ALIMENTARY CANAL.

36.—A SIMPLE METHOD OF RADICALLY CURING REDUCIBLE HERNIA.

By Dr. Julian J. Chisolm, Professor of Surgery in the Medical College of South Carolina, U.S.A.

A simple plan for radically curing hernia, which I suggested and put into successful practice in 1859, consists in sewing the columns of the inguinal ring together, subcutaneously, by silver wire, and leaving the wire permanently in the tissues, so as to act the part of a permanent internal clamp. This restores to a great extent the virgin condition of the external oblique tendon which gives strength and support to the lower portion of the abdomen. The only instrument necessary for the performance of this operation is a stiff needle five inches in length, very slightly curved towards its point, near which is placed the eye. The other extremity of the needle is secured in a firm handle, which enables the surgeon to control its movements.

The various steps of the operation are as follows:—The patient having undergone the usual preparation of having the bowels emptied by some mild cathartic, is placed in the recumbent posture, and all hair is removed from the pubic region corresponding to the side upon which the operation is to be performed. The hernial contents having been returned into the peritoneal cavity, the index-finger of the left hand is placed over the centre of the fundus of the scrotum (palmar surface upwards), the needle lying upon and parallel with it, the eye of the needle corresponding with the pulp of the finger, which can guide it in the direction it should take to the point of transfixion. The finger, with the needle now capped by scrotal tissue, is passed into the inguinal canal until the inner face of the columns can be readily felt. The pulp of the finger having passed well behind the internal column, the handle of the needle is seized, and the point, directed by the finger, is made to transfix the conjoined tendon and internal column at some distance from its free border. When the point of the needle projects under the skin of the abdomen, an assistant draws the skin inward towards the median line, so as to make the needle perforate that portion of skin which would normally lie over the central portion of the canal. The needle is now threaded with a silver wire and then drawn back into the canal and through the scrotum, leaving one end of the wire exposed upon the abdomen. If the point of the needle has escaped from the scrotal puncture, it is carefully reinserted through the same orifice, and, directed as before upon the pulp of the finger, passes with the invaginated scrotum into the canal, and is made to transfix the external pillar of the ring. As the point lifts the skin the abdominal covering is drawn outwards in such a way that the point of the needle protrudes through the puncture first made in the skin of the abdomen. The silver wire is now detached from the eye, and the needle completely withdrawn through the scrotum, leaving the two ends of the silver wire protruding from the abdominal puncture. The portion of wire embedded in the tissues forms a long loop, which extends continously through each column of the ring to the bottom of the scrotum; the extreme convexity of the loop lying in the scrotal fascia under the skin, where it can be felt by passing a probe into the scrotal puncture.

The next step of the operation consists in drawing firmly upon the ends of the wire, whilst the scrotum is drawn downwards and its invagination prevented, which forces the wire to tear or dissect up the scrotal fascia to the immediate vicinity of the ring. If the finger be now thrust up into the canal and the wire drawn upon, the finger will be squeezed by the approaching columns; and if drawn out of the canal, and the wire be still drawn upon, the ring will be so diminished in size as only to accommodate the spermatic cord, with no room to readmit the finger. The wire is now twisted from above with a torsion forceps, and when the columns are brought well in apposition, without too much traction being made to cause the wire to act as an écraseur, the ends of the wire are cut off as close as possible to the abdomen, when the portion left in the wound immediately disappears from view, under the skin.

From the beginning to the end of the operation not a drop of blood is drawn, the only external evidence of an operation having been performed being a small prick in the skin of the abdomen and a similar one in the scrotum, either of which can scarcely be found, and which heal in a few hours. For a few days after the operation the patient is kept quiet, until the wire can become imbedded in lymphy effusion. No truss need afterwards be worn, as the wire clasping the columns of the ring restores the support of the abdominal wall; the truss, moreover, would act injuriously by painfully compressing the skin against the incarcerated wire suture. One suture suffices for the majority of herniæ. Should the orifice of protrusion be of large size, as in large inguinal or umbilical herniæ, two or more sutures may be required to keep the borders of the opening in perfect apposition. A point of much importance is that of introducing the needle the second time exactly through the same orifice in the scrotal skin as it had traversed in its first introduction; for should a portion of the skin be involved in the loop of the wire, the ready dissection of the scrotal fascia cannot be effected without much force, and the scrotum becomes invaginated in the inguinal canal. The same rule holds good for the abdominal puncture, otherwise the twisted wire will not slip under the skin and become embedded in the subcutaneous fascia.—*Lancet, Sep.* 1, 1866, *p.* 231.

37.—A CASE IN WHICH A NEW OPERATION FOR THE RADICAL CURE OF HERNIA WAS SUCCESSFULLY PERFORMED.

By Arthur E. Durham, Esq., Assistant-Surgeon to, and Lecturer on Anatomy at, Guy's Hospital.

[The patient was a sailor, twenty-six years of age, who was admitted into Guy's Hospital with an easily-reducible but very troublesome inguino-scrotal hernia. He could not wear a truss.]

On Jan. 19th, chloroform having been fully administered, and the hernia reduced as completely as possible, Mr. Durham proceeded to operate in the following manner:—An incision, about two inches and a-half in length, was made through the skin and superficial fascia, in a direction at right angles to Poupart's ligament, and just over the inner border of the internal or deep abdominal ring. The tendon of the external abdominal oblique muscle was next divided in a similar direction, but to a somewhat less extent, and in a situation slightly further from the median line of the body. The lower fibres of the internal abdominal oblique or cremaster were then separated longitudinally, and the internal spermatic fascia or fascia propria of the hernia was exposed. A slight incision having been made in the lower and deeper part of this fascia, an aneurism needle was carefully insinuated through the areolar tissue, and by its means a ligature was placed between the sac of the hernia and the important structures of the spermatic cord, and carried through the upper and deeper part of the fascia. The fascia and sac were then drawn gently downwards and towards the median line of the body, and the ligature was tied tightly as high up and as far outwards as possible; in fact, as nearly as could be judged, exactly at the internal or deep ring. The ligature thus included the greater part of the circumference of the fascia propria or internal spermatic fascia just where it becomes continuous with the fascia transversalis, the whole circumference of the sac just at its junction with the general peritoneal lining of the abdominal parietes, and within the sac a small plug-like portion of omentum. In passing the aneurism needle, a small puncture was unintentionally made into the sac. This puncture, however, when subsequently dilated, afforded the opportunity of ascertaining that the sac did not communicate with the tunica vaginalis testis, but that it contained a small piece of irreducible omentum; this was drawn out and cut off below the ligature. Finally the wound was closed above and below by sutures, which were passed through the sac. The ends of the ligature were left coming out through the middle of the wound.

The after-progress of the case was most satisfactory. The patient was kept absolutely in the recumbent position for more than six weeks. From first to last he never had a single bad symptom worth mentioning. There was never any abdominal tenderness, constitutional disturbance. or other indication of general peritonitis; nor was there ever much pain about the site of the operation. The upper and lower parts of the wound healed by first intention. The ligature came away on the eighteenth day, and complete closure of the wound speedily followed. On the 29th March, the patient, having to a certain extent recovered his strength, went into the country. Before leaving the hospital he was examined by all the members of the surgical staff as well as by many visitors There could not be discovered the slightest hernial protrusion, nor any abnormal impulse on coughing. The inguinal canal from the internal ring seemed perfectly blocked by the obliterated sac and new material.

This case, although a solitary one, may be considered to prove—first, that the method of operation described is practicable; and, secondly, that it is not necessarily attended by danger. The author would add that he believes it to be more scientific in principle than any other method yet adopted, and he hopes, therefore, it may prove more successful in practice.—*Lancet, June* 16, 1866, *p.* 662.

38.—ON FEMORAL HERNIA.

By Dr. Maurice H. Collis, Surgeon to the Meath Hospital, and County Dublin Infirmary.

A very great and important change has taken place in the treatment of strangulated hernia within the last thirty years; at least, it is only within this period that the improvement to which I allude has become at all general. Formerly when a portion of intestine became strangulated much valuable time was lost before an operation was decided upon. Purgatives had to be administered by mouth and rectum, the lowering effects of bleeding, tartar emetic and tobacco, were, one or other, or all judged necessary, and prolonged attempts at reduction by taxis were repeated from time to time. Although by these means a small per centage of cases escaped operation, the remainder (a great majority) were placed under disadvantages which more than counterbalanced the benefit. In those days, according to statistics which I have compiled from various authors, the per centage of deaths, at the lowest computation, exceeded fifty per cent., and some writers, who refer to the earlier times, assert it to have been as high as two deaths in three. This great mortality was due to the combined influence of delay, of depression resulting from medicines, and of actual injury done to the intestine by the taxis. We now operate early in cases of strangulated hernia. A fair trial is given to remedies, but if they do not act promptly, we proceed to operate.

As to the mode of applying the taxis it cannot be too frequently repeated that the object of the taxis is not to return the intestine, but to empty it. Any one who studies the mechanism of hernia will at once perceive why this is so. When a knuckle of intestine comes down through a narrow opening, it gets spread out, filled with feculent matter, and gorged with blood, so that it quickly comes to bear no manner of proportion to the passage, through which, if at all, it is to get back into the abdomen. If direct pressure is made upon it when in this condition, this may flatten it a little, but cannot assist in returning it. Nay more, it increases the obstacle to return, which is produced by the disproportion between the size of the ring and the protruding intestine. The taxis is to be properly performed by laying the fingers all round the tumour, and by trying to compress the tumour by a movement which shall tend to approximate the tips of all the fingers. By this concentric movement of the fingers the intestine is gently compressed so as to unload it, if possible, first of its gaseous contents and then of its feculent, and perhaps also to diminish the amount of blood which circulates sluggishly and in a congested stream through the vessels of its walls. That this is so, all surgeons who have succeeded in returning a hernia know ; first is felt and heard the gurgling passage of air from the intestine back into the abdomen, then by continued pressure the fæces follow, and finally, the intestine is felt to slip up. Let this be borne in mind whenever taxis is used, and more especially when the stricture has been divided external to the sac.

Although chloroform is more especially useful in inguinal hernia, where the ring is more or less connected with muscle, its use is not to be despised in femoral. No doubt it cannot relax the ring, but it has great power over the abdominal muscles, and even over the muscular coat of the intestine. You have all seen how rigid the abdomen becomes when the bowel has been constricted for even a few hours. This rigidity yields under chloroform. I therefore advise you to chloroform your patient even in case of femoral hernia. In inguinal hernia this alone will often effect its reduction ; and you should use it as a preliminary treatment before speaking of operation to your patient.

In femoral, however, I have rarely found it of such distinct service until some at least of the constricting bands are divided; hence, I seek the patient's concurrence in the operation before I give chloroform, so that the one administration of the anæsthetic may suffice.

I now pass on to call your attention to Gay's operation. It is beautifully simple, and if you will take the pains to understand it you will find it as safe as it is simple. You all know where Gimbernat's ligament lies; its position is further marked out for you by the tumour, to the upper and inner side of which it lies. Feel, then, for the neck of the tumour, the spot where you cannot get your fingers under it, where it feels as if part and parcel of the deep tissues of the thigh, and on its inner side make an incision through the skin long enough to admit easily the forefinger of your left hand—from three quarters of an inch to a full inch will do. The direction of this incision, as recommended by Mr. Gay, is nearly vertical, and such a line has been usually followed in all cases that I have either seen or operated on. One objection to it arises at a later period of the progress of the case, and to this I shall now allude, although somewhat out of place. The skin in this locality abounds in lymphatic vessels, running from the pubic and infra-pubic regions to the superficial chain of glands in the groin. Any vertical or oblique incision necessarily divides many of these small and thickly-packed tubes; hence arises frequently inflammation of the glands, which may run on, as we have lately seen, into either suppurating bubo, or may be the centre of a diffused suppuration. To avoid these consecutive accidents I propose, for the future, to make the preliminary incision parallel in direction to Poupart's ligament.

In whatever direction this incision be made it is merely to go through the skin; the finger-nail is now to be used to pick up on the side of the sac every constricting fibre which intervenes between the surface and Gimbernat's ligament. The number and direction of these bands will vary according to the length of time the hernia has existed. In recent hernia there will generally be found one tolerably well-marked superficial constriction, firm enough to be taken perhaps for Gimbernat's ligament. This is formed by that portion of the superficial fascia, known as the cribriform fascia, through which the saphena vein passes to the deep femoral, and through which also at this or some other opening the hernia often escapes. This is to be divided carefully with the hernia knife. The hernial sac may then be gently compressed to see if it will yield, or it may be slightly drawn down, if possible, by one finger in the wound.

It is quite possible that such a manœuvre may occasionally succeed. Care must be taken not to use too much force. In older herniæ a succession of fibres and bands are found spread along the body or elongated neck of the sac. When all these are divided the true stricture is reached at Gimbernat's ligament; it lies wonderfully deep down, and it is to be remembered, the finger must be pushed on at right angles to the plane of the external wound, not being suffered to pass in any degree upwards, else the stricture will be missed, and the finger be lost in a region above and superficial to it. Once felt it cannot be mistaken; a touch of the knife divides it enough, it matters little whether upwards or inwards; a slight nick will do; gentle compression with one finger in the wound and the others outside upon the sac, will now empty the latter and complete the reduction. Cases will occur now and then in old herniæ where these measures are insufficient. The neck of the sac is thickened, the intestine is adherent, there is an hypertrophied mass of omentum, and you find even a free division of Gimbernat's ligament insufficient to get the hernia reduced. In such cases you should open the sac sufficiently to allow your finger in, and divide the constriction at the neck of the sac, if any exist. Large masses of omentum may be either broken up and returned, or in rare cases it may be needful to cut them away. The adherent intestine will be carefully separated from the sac and returned; in rare cases this is impossible, and you must be content to divide all constrictions freely (as is known by your being able to empty the gut), and to leave it *in situ*. As regards stricture by the neck of the sac, an old hand may even venture to nick the outside of the sac with great care, and so may avoid the necessity of opening it, but I do not advise a beginner to try this delicate and dangerous manœuvre.

You may hear many objections raised to Gay's operation; they have been refuted over and over again. Some of them arise from prejudice, others really from ignorance. The former will yield as surgeons become impressed with the superior safety of the operation; of the latter I give you but one example. It has been said over and over again, that you run the risk of returning the hernia strangulated *en bloc*—that is that you are liable to send back the sac, with its contents in one mass, still strangulated by the constriction of the neck of the sac. This really silly objection ought not to pass the lips of any one who knows how to use the taxis aright. Such an accident I never saw, nor can I conceive it to occur in the hands of any one who knows how to use the taxis properly. Gentle concentric compression, and not direct backward pressure, as I said before, is the proper method, and if this is remembered the return of the hernia *en bloc* cannot occur.—*Medical Press and Circular, July* 18, 1866, *p.* 65.

39.—AN ENEMA TUBE.

By Dr. J. Y. Totherick, Sudbury, Derby.

[Dr. Totherick considers that the following apparatus combines cheapness, simplicity, and efficiency, in no ordinary degree.]

The apparatus consists simply of five or six feet of three-eighths inch india-rubber tubing, to one end of which is fixed an ordinary funnel capable of holding a sufficient quantity of liquid, and to the other end one of the common ivory insertion pipes. The method of using it is as follows:—1st, fill the funnel with the liquid to be employed, whilst holding the exit pipe at the same level; 2ndly, squeeze with the finger and thumb the end of the pipe to which the ivory is attached, to prevent premature exit of the fluid; 3rdly, insert the ivory exit pipe into the rectum; 4thly, elevate the funnel to the length of the tube, and allow hydrostatic pressure to force in the injection.—*Lancet, Oct.* 6, 1866, *p.* 401.

40.—ON ARSENIC IN HEMORRHOIDS.

By Dr. T. Parvin.

Last summer a friend suffering with "hay-asthma," called upon me for a prescription. He likewise was a martyr to hemorrhoids, and had been for years; in vain he had submitted to the ligature, to nitric acid, to incision and to excision—each of these relieved for a time, and then the trouble returned. Fowler's solution was prescribed for him, with reference solely to the asthma. At the expiration of a week, there was no improvement in the difficulty for which the arsenic was administered, but there was a remarkable change for the better in the hemorrhoids, and a further continuance of the remedy relieved him entirely. Since that time occasional relapses have occurred, but they yielded quite well in a few days to eight drops of Fowler's solution three times a day. It is now upwards of 30 years since Sir Charles Locock pointed out the value of arsenic in the treatment of *atonic menorrhagia;* and quite recently Dr. Handfield Jones ("Functional Nervous Disorders") explains the results by the influence arsenic exerts in producing contraction of the blood vessels. Doubtless this is the way too in which it acts upon the enlarged hemorrhoidal vessels, although its application in the treatment of such diseased state is to me entirely novel, and the discovery purely accidental. That it does good I do not doubt for a moment; but that it should be resorted to, neglecting the condition of the intestinal canal with reference to proper secretion and evacuation, would only tend to discredit by failure what I believe will be proved to be a valuable application of an important remedy.—*Cincinnati Journal of Medicine.* —*Medical Press and Circular, May* 30, 1866, *p.* 575.

41.—CASES OF URINARY AND ANAL FISTULÆ, CURED WITH AND WITHOUT OPERATION.

By Dr. Andrew Paul, A.B., London.

"Can cancer be cured without the knife?" was a question asked in the leading journal of this country, some twelve years ago, by a most amiable clergyman and accomplished divine, himself a sufferer. This gentleman had been a patient of the writer ere this, for inward piles, and had got well.

A thunder-storm of professional wrath assailed this would-be-doubter of scalpel supremacy. Yet *time*, since then, has modified very much professional opinion as to the almighty influence of the knife in the radical cure of cancer.

Just so may it turn out now, on the question being asked, "Can anal and perinæal fistulæ be cured without the knife?" Professional echo will answer "No!" Here, then, reader, you have so many cases, as *naked facts*, from a dissentient voice.

Case 1.—In 1854, a young gentleman, in a government office, Whitehall, came under treatment, with five fistulous openings in right buttock, and two urinary openings midway between anus and back of the scrotum, the result of urethral stricture, caused, as he said, by strong injections.

A single-waxed string, conveyed by an eyed-probe, was passed along the main pipe of these sinuses, tied in a loose loop, and left there. The string—seton fashion—continued *in* for three weeks. Surgeons in olden times found it very hard to keep a seton in nape of the neck, from ulcerating *out*, here the intention obviously is for the string to cut *out* the parts in the rear, healing as the thread advances, and curiously enough, its presence in the main tunnel, aided by water aspersion, percussion, and injection, into and against the surrounding parts—hot, when tender; cold, when not so—not only obliterated the one wherein it was, but the four tributaries as well. The enlargement of the right buttock, which, from long-continued burrowing of matter, had become twice the size of its opposite neighbour, subsided. The urethral discharge, with which the patient was literally flooded, ceased by catheterism of water percussion, and aspersion along the floor of the urethra, the stricture gave way, and, in time, the urinary fistulæ also dried up. And cold water injections materially helped in altering the diseased structures around. What one of us who would not rinse out his mouth after teeth-cleaning? so, likewise, the bowel, for health and cleanliness.

Case 2.—A gentleman, aged 30, called in the autumn of 1855, with urgent symptoms of retention, consequent upon stricture, in a state a spasm, the result of excessive drinking the previous night. Three years before had been treated by caustic injections for urethral discharge, that had stuck to him for six months. He begged to have an instrument passed, but was put off with the assurance of a more speedy and less painful method of immediate relief. He was seated over a shower of hot poppy-head water, from a douche-jet No. 1, for about twenty minutes, when, to his amazement, a stream of water flowed from him almost involuntarily. He was desired to continue this daily and nightly for a month. Has since indulged in after-dinner libations with impunity, relying upon aspersion alone to anticipate or to meet the urgency of an attack; and has latterly been using cold aspersion with an amount of benefit such as has insured to him release from an attack ever since. This case, bearing as it does upon the previous one, is given as a proof of local anæsthesia from warmth.

By adopting this simple course, at the same time applying cold per anum, as by lavement, not only will the impression of this salutary agent be conveyed through this channel — the rectum—to the bladder, but the bowels will be kept thereby in an easy comfortable state. Let any one afflicted with incontinence of urine from catarrh of the bladder, from prostatic disease, or from uric acid, or alkaline in excess—above all, from chordee—inject, were it but a wineglassful of *cold* water, into the bowel, he will be astonished by the effects, from the instantaneous relief experienced through the agency of this powerful sedative.

Case 3.—A gentleman in the city bank, April, 1856, has had gonorrhœa thrice. The last time treated with stimulating injections. A fortnight after getting well, felt uneasiness and pain in the perineum. An abscess formed and was opened by a surgeon, but the urethra was not then explored. The abscess healed up and cicatrized. It opened again the following January, and notwithstanding caustic in substance and astringent lotions, the outlet had not closed up at the time of his call. He said he wished to get well soon, as he was to be married at Midsummer. Catheters Nos 5 and 3 failed. Having used hot water aspersion for twenty minutes, No. 10 catheter passed into the bladder with the greatest ease. On his subsequent visits, the catheter invariably passed, as at first aided only by the hot water jet. The next step was to close the fistula, and to cure the gleet, which off and on stuck to him from the first. He was enjoined to use aspersion, if possible, *before* passing water. On two occasions a probe, with a hole and string through its probe-end, and dipped in a weak solution of sulphate of zinc, was passed down the fistula as far as it would go. The result was that, instead his shirt being wetted the size of one's hand, the urine stained a surface not larger, at first, than a crown; nor, secondly, than a sixpence; ultimately he passed water without one drop escaping through the opening.

Case 4.—May, 1864, Mr. W., from Brighton, called. Fistula in left perinæal region, leading towards the anus. Passed an armed probe, which, on reaching the lining membrane, was made to pass through into the gut, and then hooked out, the probe being unthreaded, was withdrawn, and the string tied loose. Fomentations relieved the stiffness that ensued for a few days. Mr. W. was enabled to attend business, and occasionally to come to town. Refusing to have the string tightened, or changed for a new one, the time taken to work out extended over six weeks. Sea water injections were used daily. The case ended in a cure.

Mr. W. had been cut for fistula on the right side, by the late Mr. Lawrence, and had been three weeks in bed.

This case could be traced to no other cause save to regular autumnal diarrhœa, which for years he had been subject to.

Case 5.—Mr. C., aged 52, called in August, 1856; a builder by trade He never knew what illness was that needed a dose of medicine, therefore averse to taking any.

A solid tumour the size of a marble was felt in the right buttock, with a "core" leading towards the gut, full an inch and a half from the outlet; this was tender to the touch, no evidence at this time of matter having burst into the gut, as there was no pain at or after the closet, nor was there on the stool the slightest vestige of matter. Anal injections—warm when tender, cold after escape of matter— were the only means used during nine months; healthy action being set up, abscesses ceased to form, the opening closed, and all induration became absorbed.

This gentleman, who was seen twelve months ago, reports nothing amiss now.

Case 6.—In 1864, Mr.——, mayor of a borough town in one of the midland counties, applied for advice. Fistula underneath the fold of left buttock, leading towards the gut, which it did not penetrate. An abscess had formed nine months before, outlet closing and opening as matter formed and ceased to form in surrounding tissues. In the centre of the right buttock was a second opening, leading straightforward to a depth of three inches into the buttock cushion, but unlike the other, not leading towards the gut. His health had been impaired. Scarlatina had entered his family six months ago; had been both mild and malignant in its form. My patient had it in the throat severely; since then, health indifferent; mind depressed; was advised to have both fistulæ laid open; objecting to this he commenced cold water aspersion, percussion, and injection; in the latter the *stream* of water against the anus so relaxed the strictures as to pass in unaided by elbow-pipe, thus creating desire for stool, and to rinse out the rectum as well; this course has been pursued ever since.

For the last six months I have not seen this gentleman, though I have heard of him through the member for his borough, whose wife I have since attended for inward piles.

Case 7.—Mr. M., aged 30; strongly marked scrofulous diathesis; scars in various places, neck and chest; fistula lachrymalis of left eyelid years ago left a prominent scar; three openings in left buttock, two in right: not one leading into the gut; complained of palpitations, headache, sleepless nights, loss of appetite, occasional night-sweats; all tonics making, as he said, his head worse; nothing in the way of medicine was taken. His office-hours were light, not over-confined to the desk; still he had to breathe in an atmosphere which he found prejudicial to health—namely, that of a wholesale tobacconist. The seton healed up four of these sinuses, still fresh collections of matter formed; as a last resource, I said, "to Margate forthwith; live well, and as much on the water and in the sea-air as you can; bathe also." He did so, in three months returning, with all the sinuses dried up; health and strength restored; since 1864 has been two voyages to America and back, as purser in a steamship company, wherein the writer procured for him a berth.

Case 8.—In August, 1865, Gunner R. Jury, Royal Artillery, Maidstone, came under treatment. A twelve-month before had been in hospital, and in bed at Woolwich six weeks, having been cut three times in the right buttock for complete anal fistulæ. The string took longer than usual to work out. Why? Because of the parts being indurated through the previous operations. On the left side there was an incomplete fistula running to, as on the other side towards, the rectum. The latter I treated with seton in the presence of Assistant-Surgeon Boulton, to whose kindness and professional courtesy I feel much indebted. This case has got well, under the care of Surgeon B., to whom luckily, at the time, a similar case presented, and who introduced a string, with similar results.

Case 9.—Quarter-Master Sergeant G. Ripley, at the same station, and in the same troop as foregoing, applied to me for fistula on right side, and inward piles on the left verge of the anus. I passed a seton for the sergeant; duty in the saddle called him on parade next morning, stiff though he was, yet he went through an hour's drill. Was excused from similar duty but one morning all through the treatment. His *faith* in cold water aspersion and percussion was such that he never omitted to use it both before and after saddle exercise; this has likewise been successful. The inward piles now trouble him but little. The cure of fistula will often cure piles. These cases have been reported on to "head quarters," by the senior surgeon of the depôt, as evidences of treatment quite compatible both with the ordinary duties of the soldier, who is not necessitated to forego them, and as not requiring confinement to the wards of a military hospital. The attention of the military authorities has also been directed to the treatment, as the means of saving to this arm of the service many valuable men, who otherwise would be discharged.

Treatment. — 1st, let us 'consider the *principle* on which it rests: 2nd, the *process* by which it is accomplished.

The structures in and around the anus are, even in the normal state, but of low vitality, and when diseased, are most prone to suppurative inflammation. It is extraordinary the extent of mischief entailed on this locality by neglect or by untimed treatment. Three such cases could be adduced were it necessary; one, however, is given briefly as a striking instance how mind and body become victimised. In 1838 a gentleman called from Yorkshire; repeated abscesses had formed; tardy ill-timed openings had been made; suppuration and sloughing from time to time had so destroyed the tissues surrounding the anus that a cricket-ball could be buried in the *gulf* between the tuberosities of the ischia; the anal sphincter had been destroyed, and though all was healed over, yet on the slightest error of the stomach or bowels, wind, gelatinous exudation, and liquid fæces, would escape. His object in calling was to procure some mechanical means of resisting fæculent escape. Though a family man, with ample means, yet so sensitive was he on the score of effluvia, as to live in a separate wing of his mansion apart from his family, still so personally, so scrupulously clean, that the most *fastidious nose* could detect no smell. A spring-bandage and lint pad, with aspersion and injection, so far obviated the nuisance complaned of, that this patient, on calling a twelvemonth after, assured me everything had succeeded. The pillars of the rectum had assumed the office of sphincters, and that, excepting when relaxed, he gained complete control over the exudation and had returned to family society. Before we met he declared he was barely passing through life, "now (on his second visit) I'm enjoying it." But to return, a new and *long-continued* action must be set up. Those who fancy the knife must admit that in twenty-four hours after the operation all irritation sinks below par, requiring caustic in substance, daily and nightly dressings, relays of poultices—in hot weather most offensive—strong astringents, chloride of zinc, Condy's fluid, all combined, caging the patient for weeks—nay, months—within the walls of bed or dressing-room; whereas with string or seton, long-continued healthy action, aided by the other means, is kept up, and this at the cost of but trifling inconvenience to the patient in the pursuits of every-day life. Such is the experience of thirty years. Nor can it be said with truth that the string is much in the way during the calls of Nature. Even the act of cleansing is productive of good, as the seton is moved and drawn upon, so as to allow the escape of matter should the string cause lodgment. Once the wall of the gut becomes perfect by healthy granulation, no further escape of gas, of fæces, or of the bowel's exhalation can escape into the structures around; hence a stop to suppurative inflammation. In short, after the string has *cleared* the bowel, and the slit has been filled up by granulation, the *bore* of the gut again becoming *patent*, neither gas, nor fæces, nor exudations, can escape therefrom. The seton enters upon its second stage—namely, to rectify and to restore to health the structures *outside* the wall of the gut, which to a certainty it will do the looser it is, the longer, therefore, will it remain in the flesh. If tightened every other day, the seton will work out in ten days, quite too short a time for the object in view—viz., restoration to healthy from *un*healthy structural action, the surgeon will be beat and his patient annoyed. For what happens? The superficial parts bridge over by too hasty adhesion, the tunnel underneath remains, the surgeon has to break up the adhesions on the surface, some stimulating ointment must be applied, a torpid state of parts sets in, just as after the knife performance, and time is lost. The writer has often noticed that some patients' flesh will *rot* the string much sooner than others. So was it in the gunner's case. The seton had to be replaced thrice; whereas, but lately, in the case of Capt. S. (fistula having followed dysentery in India), the second string, when withdrawn, was found as tough as a trout-line, which the Captain, another Isaac Walton, assured me would hold a good-sized fish. In the case of Mr. W., Brighton, the seton was not once changed.

2ndly. The "process," or method of getting the seton in. In all these cases of fistula, whether anal or urinary, there is invariably undue crampy-action of sphincters, or of the urethral muscles; hence extreme tenderness, often amounting to torture, whether from the finger introduced, or from catheter ever so gently passed, the hot water aspersion, or, as recently, the "ether spray," for five or ten minutes, will allay all spasm; a simple silver probe or fine gum-elastic catheter, tipped with a silver bulb, having an eye drilled in the probe end, is passed along the fistula. On its reaching the gut there, the forefinger of either hand passed into the bowel will encounter the instrument and will hook it out. The eye of the probe is then threaded with an ordinary shoemaker's "waxed end," to which a bristle has been secured. This bristle helps the surgeon vastly to thread his probe, the waxed end is drawn through the probe eye, so as to double the string or not, as the surgeon thinks fit; the probe is withdrawn, first, back into the gut; secondly, out from the fistula. The probe is cut off, and the surgeon ties his loop, tight or slack, with an ordinary reef-knot, cutting off the ends of the waxed string, so as to be out of the way in cleansing.

If the surgeon wants to change his string, all he has to do is to loop on over the knot a second waxed end, the knot will keep the new string from slipping. With a dissecting forceps he draws the circle of the original loop round and round until the knot, holding on the loop of the new string, clears either the fistulous or the anal outlet, he then cuts the old string, and with the new one makes a new loop. Should the surgeon be summoned to a case without knowing beforehand what is the matter, if he has not got his probe in his pocket, rather than probe a fistula twice, let him ask for a lady's bodkin, which, if silver, supple, and with an eye in its bulb, will answer just as well as a Weiss's or Fergusson's probe, three or four strings of whity-brown thread, well-waxed, will answer his purpose: thus the surgeon kills two birds with one stone, he probes his fistula, and, at the same time, he passes his string to cure it. If the anal fistula be incomplete—*i.e.*, not entering the gut—the moment his finger in ano meets with the head of the probe, he can scratch with his nail an opening for the probe to pass in, making the the probe a fulcrum for the finger nail to act on.

Remarks.—Incontinence of fæces—a rinderpest plague-spot to the knife, has not followed in one single instance upon the foregoing method.

That fissure, fistula, both anal and urinary, can be cured without the knife, has been, as it is to the present hour, amongst bygone and modern surgeons a *vexata questio.* To this, historical surgery, both periodical and voluminous, amply testifies. The "perineal section" has been a *casus belli* amongst moderns, north, south, east, and west in the British Isles.

Indeed, one doubts whether an individual advocating or venturing to advocate, a peaceful mode of treatment is, or is not, an othodox member of the "war conference." However the writer trusts that ere the "treaty" be signed the working of his principle will have a further and fairer trial. As in military so in surgical warfare, routine must eventually give way to mishaps. Years may pass ere such be the result. In meantime, we must be content to escape, if we can, the casualties which "system" entails upon us. Should we, like Shadrach and Co. of old, escape scatheless from the fiery furnace, *fortune* indeed has smiled upon us.—*Medical Press and Circular, Aug.* 1, 1866, *p.* 117.

42.—ON A CASE OF DOUBLE PSOAS ABSCESS, WITH WOUND OF THE FEMORAL ARTERY.

By John Adams, Esq., Surgeon to the London Hospital.

[Mr. Adams relates two cases, in the first of which the opening of the abscess was followed by death in forty-eight hours, in the second a cure resulted, thanks to wine, quinine, good diet, and rest in the semiflexed position for many months.]

I think I am right in the conclusion to which I have arrived from the observation of many cases that, where the abscess depends on desease of the bodies of the dorsal vertebræ, the cause is more likely to happen than if the lumbar vertebræ are affected. I presume that in the dorsal region there is a more natural tendency to coalition of the bones from the direction of the natural curvature than in the lumbar region, where the bodies of the vertebræ present a natural convexity forwards. In the last case I do not hesitate to say that the disease was in a very great measure cured by position of parts; but of course a great deal depended on the stimulus and good diet this young woman enjoyed in the hospital. In the case before us the disease has been of very long standing, and the vertebræ have become fixed, and the abscess is probably consequent upon some access of inflammation in the body of one or more of the dorsal vertebræ, as indicated by pain at the seat of curvature and general indisposition. You will have observed that both abscesses had remained stationary for a very long time, and that all my efforts were directed to the improvement of the boy's general condition, for this purpose sea air was recommended, and he was expecting daily to be sent to Margate. However, on my visit on June 2nd, I found that the abscess on the right side had suddenly enlarged, and the enlargement was attended with great pain and tension. I therefore proceeded to open it, taking, as I presumed, the greatest possible precaution to avoid the femoral artery, which is so often displaced in deep abscesses. Unfortunately, however, I not only punctured the artery but I actually slit it up for the distance of a quarter of an inch. I need not say that the escape of arterial blood in a rush, and with an audible whizz, informed me of the fact, and I at once enlarged the opening in the abscess on my forefinger, seized the structures between my finger and thumb of the left hand, and asked my colleague Mr. Maunder, who was present, to secure the artery with an armed aneurism needle. This was done with great facility after he had laid bare the artery on my forefinger. When the ligature had been applied above the wound I sought for the opening in the artery and directed another ligature to be applied below the opening. I felt now that everything was secure, the limb was enveloped in cotton wool, and treated as after ligature of the superficial femoral for popliteal aneurism.

Let us now pause and briefly consider this case. Where was the abscess which had thus dislodged the femoral artery and raised it from its bed? Clearly it had made its way in the course of the psoas, but instead of pointing superficially, as it does most commonly, it had taken a direction deeper than usual, and most likely had made its way, as the muscles does, downwards towards the trochanter minor and along the front of the femur under the fascia lata. Anatomy teaches us all this with great precision, and believing in the possibility of such an occurrence, I had actually made the remark that before opening deep abscesses like the present, the surgeon should satisfy himself as to the position of the femoral artery. I did with my own fingers try to feel the artery, but I failed to do so. Its pulsation was no doubt feeble, and most probably the tension of the abscess from below rendered it quite imperceptible. I cannot say I erred from ignorance; but I am free to confess that I should have been more cautious. But having opened a large artery you must proceed to tie it, for no pressure, however judiciously applied, is sufficient to prevent a recurrence of hemorrhage usually; and although exceptional cases may now and then occur, believe me there is no security against bleeding equal to the ligature when a large artery is wounded. In this case no pressure could be applied, as there was no solid body in the vicinity of the artery.

What is the treatment to be pursued in psoas abscess? Put this question to yourselves and think the case over. The disease here evidently is in the spine, the abscess is its consequence; therefore, put aside the question of the abscess and try and cure the disease on which it depends. If you can do this, the abscess may disappear, either by becoming absorbed, or you may open it. That abscesses do disappear I have no doubt. The case illustrates the position, for during the treatment of the abscess in the right thigh that in the left has altogether disappeared. I remember a case, under the care of Mr. John Scott, formerly a surgeon of this hospital, in which a psoas abscess suddenly disappeared, and the urine was loaded with pus for some days; clearly the abscess burst into the bladder or the ureter. The patient got well. It is, however, advised to let the abscess alone as long as you can, either hoping for its disappearance or deferring the opening until absolute necessity demands your interference by operation. No man likes to meddle with such cases unnecessarily.

The proper time for opening the abscess is when tension and its consequent pain render the case unbearable. How, then, are you to open psoas abscess? This question implies that there is some risk in the operation, and this really is the case. The risk is lest the patient should become exhausted by long-continued discharge, and that hectic fever should arise from inflammation of the cyst of the abscess consequent on the admission of air. Hence the efforts of surgeons have been directed to provide against this most frequent contingency. Mr. Abernethy advised that a small valvular opening should be made, the matter to be allowed to escape without pressure and by the elasticity of the sac itself, and the wound to be carefully closed by adhesive plaster; and after a short time, before the sac had refilled to its original size, that the puncture should be repeated, and a similar treatment again pursued as after the first puncture; and he hoped that by these means the cyst might contract, and the abscess might thus disappear. The plan rarely answers, and the reason is obvious—the disease of the spine still remains, and unless that gets well, which is most rare, the discharge must continue, and will, probably, eventually wear the patient out.

My advice is this—When you think it desirable to make an opening for the discharge of the matter, make one of moderate extent with a sharp pointed bistoury, place a fomenting cloth over the opening, permit all the pus to drain away without pressing and by the elasticity of the sac itself, and after this apply a soft linseed poultice over the wound. You may use a drainage tube if you like, but the objection I have to its employment arises from its acting as an irritant in many cases, and thus increasing the discharge it was employed to diminish. Never lose sight of the general treatment of the case, by position, tonics, stimulants, and especially by sea air, or, if this cannot be obtained, residence in the country. Machines for the conveyance of patients by rail are so beautifully contrived that no damage need result to the spine itself in travelling; or passive exercise, so to speak, may be enjoyed with the greatest ease, in the recumbent posture on the sea beach without the slightest fatigue.

But in these cases which result so unfortunately, remember the golden rule, *Principiis obsta*. Arrest the disease when it is just beginning. Remember that long before any advance has been made symptoms will have presented themselves which ought not to have been passed over, as pain in the loins, weariness in walking, emaciation, slight febrile symptoms, dyspepsia, constriction around the chest and abdomen in the course of the spinal nerves, and others of this character, which are the certain harbingers of mischief beginning in the vertebral column, which eventuates in that most intractable affection we are now considering. This is the period for the employment of issues, rest, tonics, &c., &c., and, at this time, if steady use of these means is enjoined you may fairly expect to ward the disease off altogether.—*Medical Press and Circular*, *Aug.* 1, 1866, *p.* 113.

ORGANS OF URINE AND GENERATION.

43.—ON THE ENDOSCOPE AS A MEANS FOR THE DIAGNOSIS AND TREATMENT OF URETHRAL DISEASE.

By Christopher Heath, Esq., Assistant-Surgeon to, and Lecturer on, Anatomy at the Westminster Hospital.

For the benefit of those who may not have seen the endoscope, I may explain its nature in a few words. It consists of a paraffin lamp enclosed, for convenience, in a wooden case, from which the light is conducted through a lens on to a perforated mirror set at an angle in a cylinder. An eye-piece is adapted to one end of this cylinder, and to the other a screw, by which the various tubes which are to be introduced into the several outlets of the body may be attached. I have three tubes for the urethra, corresponding to Nos. 12, 10, and 8 of the catheter gauge, and which for convenience I call Nos. 1, 2, and 3, each measuring eight inches in length, and, being provided with an aperture on one side near the outer end, through which a wire, carrying cotton-wool, can be introduced into the interior of the tube, and be made to reach its extremity so as to wipe away any discharge from the surface under examination, or to apply medications to it. A larger tube, of similar make, is very efficient for examining the rectum ; and a tube with a piece of glass at the end can be used for examining the interior of the bladder, the glass preventing the entrance of water into the tube The light, which is very powerful, and quite under control, is directed by the oblique mirror to the end of whatever tube may be employed, and the eye of the surgeon is able to look through the centre of the pencil of light without being dazzled, and to observe whatever may be brought into the field at its extremity.

I have been able to use the endoscope satisfactorily on the urethra, bladder, and rectum, but shall confine this paper to my observations on the urethra, and particularly with relation to the treatment of gleety discharges In using the instrument for the examination of the urethra, the most satisfactory position for the patient is the recumbent one on a table or sofa of good height, with the knees bent over the edge and slightly separated. The surgeon then introduces the largest tube which the meatus will admit, and passes it gently until he meets with an obstruction, or finds from the direction of the instrument that he has entered the membranous urethra, when it is well to pause lest on withdrawing the plug (which much facilitates the introduction, as with a vaginal speculum) a gush of urine should take place. The tube is now attached to the cylinder of the endoscope, which is placed horizontally, and the light being adjusted, the operator proceeds to look through, when he will see the surface of the urethra at the end of the tube shutting down upon it with perfect regularity if healthy, and by slowly withdrawing the instrument, with his eye still fixed at the cylinder, he will be able to note the various modifications which the urethra may have undergone in its whole length, if necessary afterwards examining the prostatic portion.

Although practice on the dead body is useful at first for studying the manipulations, it is necessary to observe the living urethra to gain a knowledge of the appearance of the mucous membrane in health ; for, as both Desormeaux and Cruise have pointed out, the pallid appearance of the anterior part and the dark purple of the bulbous portion of the urethra, as seen in the dissecting-room, are due to post-mortem change, the healthy urethra in life being of a bright pink throughout, and only very slightly deeper in colour about the bulb. For the sake of contrast, although it is impossible to convey the full idea without colour, I have given a sketch of a healthy urethra as seen with the endoscope, and it may help the appreciation of it if I say that it resembles very closely, on a smaller scale, a healthy rectum as seen through a speculum, or a vagina, but with the difference that the folds of mucous membrane of the healthy urethra are longitudinal instead of tranverse. I have noticed in perfectly healthy urethras that there is a constant vermicular contraction of the wall of the canal, apparently passing towards the bladder ; and this accounts for the well-known fact that foreign bodies in the urethra tend to pass in that direction. This is due, of course, to the muscular fibres of the urethra, and is wanting in cases where any induration exists. The mode in which an indurated and strictured (even though very slightly so) portion of the urethra closes down abruptly on the end of the tube as it is withdrawn is very characteristic, and contrasts strongly with the gradual closure of the healthy canal.

Great gentleness is necessary in introducing the urethral tube to avoid the induction of hemorrhage ; and, indeed, it is hardly possible, in diseased urethras, to avoid a few drops of blood, which obscure the view at first, but can be readily removed by the careful introduction of a pledget of cotton wool by means of one of the wires I have previously mentioned. Where hemorrhage has exceeded a few drops, so as to form a coagulum in the tube, it is, I find, better to withdraw the tube and cleanse it, than to attempt to sponge it out with cotton wool *in situ*, since the clot is very difficult to extract satisfactorily, and is apt to be pushed on into the urethra. The cotton wool, if not carefully twisted on to the wire, may become detached and be left in the tube, or beyond it in the urethra. It may be fished out with a hook supplied for the purpose, but even if left in the urethra it does no harm beyond stopping the inspection for the time, since it is always passed on the first occasion of making water.

The application of a solution of nitrate of silver to its actual seat is one of the most useful methods of treating the disease revealed by the endoscope, and it is readily accomplished by one of the cotton-wool sponges I have described. The solution I have invariably used is that recommended by Desormeaux—namely, twenty grains of the salt to a drachm of water (1 to 3). This, although so much stronger than any of the ordinary urethral injections, excites no disturbance when applied to the diseased surface, and ordinarily gives nothing more than a momentary smarting pain. I have never seen any inconvenience arise from its use, although, as might be expected, it sometimes increases the discharge for a day or two. The use of injections, and of internal remedies, must be had recourse to as well, however, in order to effect a cure, in the majority of cases.—*Lancet, Oct.* 13, 1866, *p.* 408.

44.—ON A NEW INSTRUMENT FOR THE CURE OF STRICTURE OF THE URETHRA.

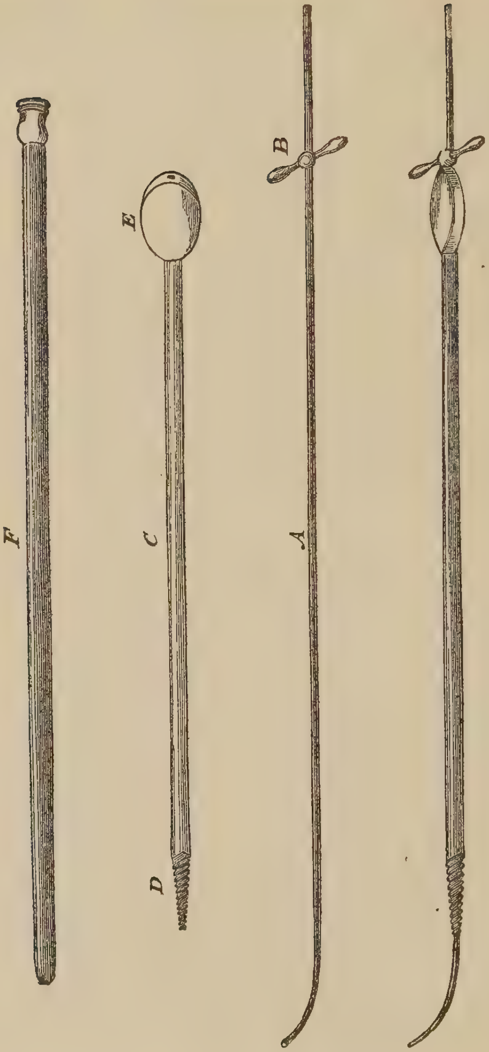
By Dr. Charles Owen Aspray, Newton-road, Westbourne-grove, London.

As regards safety in the cure of stricture of the urethra, the old system of gradual dilatation stands first; in fact, with it there is no danger if proper care be taken; it is, however, a tedious process, taking weeks, months, or, it may be, years. The want felt of a quicker and perhaps more curative method has brought into being many dilators, most of which, although they act at once and with some precision, are yet attended with a considerable amount of danger and uncertainty in their use. Some of these instruments have great power, but simply burst open the stricture, the rupture generally occurring on the side which is most healthy, the fibrous tissue of the stricture being stronger than the healthy structures. Others are of no use, simply from a want of power.

The instrument which I wish to bring under the notice of the profession will, I think, combine the safety of the gradual method with the quickness of action and speedy cure of the rapid dilatation principle. I believe also it has some great advantages over instruments used of late for this purpose—viz.: 1st. As soon as No. 1 is passed, dilatation can be commenced without withdrawing the instrument, which is often replaced with great difficulty. 2nd. The extent of the dilatation at one sitting is left to the discretion of the operator, and a bougie, the size of the dilator used, can be passed into the bladder without withdrawing the first instrument, which may then be removed. 3rd. The power is sufficient to dilate any stricture, and can be used without the abrasion produced by the sliding tubes advised by many operators.

The instrument, kindly manufactured for me by Messrs. Mayer and Co., of Great Portland-street, from my own model and drawings, is composed of a No. 1 catheter (A), having a little cross bar (B), which serves as the handle, and can be removed when a screw is loosened. When this handle is withdrawn, the dilator (C), having the conical screw (D) at the end, can be passed over the catheter, and worked by the handle (E). The dilator may be the size of No. 6, 10, or 12, and has a thin metallic cover reaching from the handle (E) to the screw (D), working smoothly over the tube beneath. This is for the purpose of steadying the penis while the screw works in the stricture; it also prevents any abrasion about the anterior portion of the urethra. The bougie (F) is passed over the catheter into the bladder after the dilator is withdrawn, and will be especially applicable where a small dilator has been used; it may be left in from twelve to forty-eight hours, according to the wish of the operator. It will be seen from the above description that the principal novelty in the instrument is the use of the conical screw for the purpose of dilatation.

The catheter No. 1 is first passed into the bladder (which of course should be made certain by the urine passing through it); the handle being removed, the screw of the dilator is freely oiled and passed down the urethra until the obstruction is felt, when the penis should be firmly grasped by the hand, and pulled down over the dilator while it is screwed through the stricture. The operator must be careful during this part of the proceeding, not to push the guiding catheter with the hand; and next to unscrew the dilator when withdrawing it. If this be attended to, the operation is done without pain; but, if the instrument is pulled without any rotary motion, it will require some force to disengage it, and be painful to the patient. The dilator being withdrawn, the bougie is pushed into the bladder over the No. 1 catheter, which is taken out, and the bougie allowed to remain. I have found a bougie one or two sizes smaller than the dilator to be of equal benefit with one of the same size; it is less trouble to pass, and produces no pain or abrasion. The bougie is of great service where there is likely


F
E
C
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B
A

to be much congestion after the dilatation, and also in cases of urinary fistula, in which the instrument, I think, will be especially useful in affording a rapid and certain means of permanent cure. As far as my experience yet goes, I think that in the majority of cases, perhaps in nearly all, the bougie is not required. Some little congestion is present for about twenty-four hours after the dilatation; and I find it better, if the bougie is not used, to avoid the trial of a catheter for some days (*i.e.*, some little time after the congestion has entirely subsided), when it is often the case that one a size or two larger than the dilator used will pass with ease. Thus in Case 1 it will be seen that, although some difficulty was experienced with a small catheter two days after the operation, yet a No. 8 passed easily on the sixth day, the size of the dilator used being only No. 6. I believe the instrument, if properly used, produces just enough congestion and absorption to cure the stricture without any danger. I add the outlines of three cases.

Case 1.—J. C——, aged sixty-three, labourer. He has suffered from stricture of the urethra twenty-six years. The patient was operated upon at St. George's Hospital eleven years ago; after that he passed his urine in a large stream for some time. He has had treatment occasionally since. From his own neglect he now passes water only by drops, and very frequently in the day time; he also gets up many times in the night for the same purpose.

Dec. 17th. Cannot pass any instrument.

18th. Succeeded in passing a No. 1 guide; used dilator No. 6. The stricture is situated at the bulbous portion of the urethra, and is of considerable breadth. The bougie not used; but the instrument withdrawn directly. Ordered, tincture of opium, twenty minims; chloric ether, fifteen minims; camphor mixture, an ounce and a half: to be taken at bedtime.

19th. Has been out of bed many times in the night, and passed water with difficulty.

20th. Passed a No. 5 catheter down the urethra; but, finding considerable obstruction, withdrew it immediately. After this he voided his urine in a stream, which gradually increased in size; but I made no attempt to pass an instrument until Dec. 24th, when No. 8 passed into the bladder without the slightest difficulty. Thus in one week the stricture was cured without any bad symptoms.

Case 2.—J. A.——, a gentleman aged twenty-eight. Has had gonorrhœa formerly, only once, but frequently had a gleety discharge since. He has noticed that his stream of urine has been twisted and smaller than natural for four or five years; it is now very minute, and he has had occasional attacks of retention during the last twelve months. He also complains of scalding during micturition, and some discharge. The urine is very acid, and deposits a large amount of urate of ammonia. He is obliged to pass urine eight or ten times in the day. Ordered, twenty-grain doses of the bicarbonate of potash three times a day for a week.

April 20th. He does not micturate so often. Has less discharge and scalding. Passed a guide, used a No. 6 dilator, and left in a No. 5 bougie. Ordered, twenty minims of the sedative solution of opium immediately.

21st. Has felt little inconvenience from the bougie, which I withdrew.

22nd. Passes urine in a larger stream than he has done for years.

25th. Passed a No. 7 catheter.

The catheterism was repeated every three or four days, a larger instrument being used each time until May 5th, when I passed No. 10, and have used that sized instrument at increasing intervals ever since. He is now in perfect health, and is never troubled with any symptoms of stricture.

Case 3.—(A very small stricture complicated with chronic cystitis). W. P.——, aged thirty-two, blacksmith. Says he has had difficulty in passing his urine for the last five years. The urine now dribbles from him constantly, and is loaded with pus. I failed on three occasions to pass an instrument.

May 3rd. I succeeded in passing a No. 1 catheter into the bladder, and used a No 9 dilator. I left in a No. 7 bougie, and gave an anodyne draught.

4th. He has passed a restless night. Bougie withdrawn. Voids urine in a stream the size of No. 6.

9th. Health much improved. The patient says his stream has increased in size every day.

10th. Passed a No. 9 catheter. The urine contains little pus now.

13th. Passed No. 10. Goes to his work.

June 17th. His urine is free from pus. He has increased considerably in weight, and has never any trouble with his urine. A No. 7 staff is passed once a fortnight.—*Lancet, Aug.* 11, 1866, *p.* 146.

45.—ON THE TREATMENT OF SEVERE STRICTURE OF THE URETHRA BY OVER-DISTENSION.

By Henry Thompson, Esq., Surgeon-Extraordinary to H. M. the King of the Belgians, Surgeon to University College Hospital.

About three years ago I placed on record, at the Royal Medical and Chirurgical Society of London, an account of a new method of treating obstinate stricture of the urethra. I have now had an increased experience of its capabilities; and I desire here, in few words, to describe the principles involved in the procedure, and the details necessary to be attended to in its application.

First, there is one very important fact which is in intimate relation with all treatment of urethral stricture; and it has been as far as my observation extends, somewhat overlooked, or at all events it has not, I think, affected so prominently as it ought the surgical treatment of the malady. It is this: *that portion of the urethra which is most frequently affected by stricture possesses in its natural condition a calibre at least double that of the external meatus.* It follows then of necessity, that any kind of dilatation of the strictured portion which is limited in extent by the size of the external meatus does not half restore the natural calibre of the canal. In other words the external meatus having in any case a diameter equal to that of No. 12 of our catheter scale, the natural diameter of the bulbous portion will equal at least that of No. 16 or of No. 18—an area of more than double that of the former. Hence, if this latter portion becomes the subject of stricture, and the dilatation is carried no higher than No. 11 or 12, the stricture is not more than half dilated.

Practically, for the great majority of cases, such dilatation suffices for all purposes. The patient loses all his symptoms, and continues well on condition that he keeps open his stricture by occasionally passing an instrument. Happily, ordinary dilatation effects all this for most patients. For a few exceptional cases, however, it is insufficient to relieve the symptoms. The passing of a full-sized instrument does not enable the patient to pass even a tolerable stream; it often produces absolute retention, or rigors, or increased pain. For such a stricture we are accustomed to use some more efficient proceeding—e.g., to divide the hardened tissues (urethrotomy), or to burst them at a single operation (rupture). Each proceeding has been, and is, of considerable value when judiciously adapted to the case.

But I wish to remark that by neither of these operations is the urethra restored to its original calibre at the point of stricture. So long as the rupturing instrument or the dilating instrument is limited in its size by the calibre of the external meatus, the contracted portion of the urethra is only half dilated.

It is due to Reybard to say that he, perhaps more than any other surgeon, has recognised the necessity of bearing in mind this important truth. Advocating about the year 1850 extremely long and deep incisions as the treatment of stricture, his system soon fell into disrepute. He not only made these incisions to an extent which I believe to be both unnecessary and dangerous, but he was careful to keep apart the borders of the incisions for a long time after the operation by means of an instrument with two blades, which could be widely extended after their introduction into the urethra.

Now my own experience has convinced me that in many of these obstinate cases an equally perfect result may be accomplished by means of a powerful distending instrument without any incisions at all. The principle which underlies and regulates this proceeding is that on which I lay some stress—viz., to restore, if possible, the bulbous urethra to its natural size, regarding it as about twice that of the external meatus.

In order to effect this purpose, I have designed and employed an instrument which somewhat resembles others that have gone before it, but which have been differently used; such, for example, as that referred to above, and also some employed for gradual or slow dilatation, applied at intervals of two or three days during a considerable period of time. The main and the important distinction consists in the method of employing it; in effecting the object by a single application only, instead of by numerous repetitions. My instrument, however, is much more powerful than its predecessors. It opens the bulbous urethra to more than double the size of the meatus, and acts mainly on the strictured, not on the healthy, portion of the urethra. When closed for introduction, it equals in size about a No. 5 catheter. As many strictures on which it is necessary to act are smaller in calibre than this, it is then essential to tie in a small gum catheter for thirty-six or forty-eight hours before employing it, in order to dilate the stricture temporarily to a sufficient size to admit the instrument. It is assumed, of course, that the stricture has been proved not to be permanently benefited by such (i.e., tying-in) dilatation, or the treatment about to be described would not be adopted. However narrow or retractile a stricture may be, it is always possible, by tying in, to dilate temporarily as far as to the required calibre.

The next principle which guides the treatment is this. Experience shows that most living tissues, if over-distended, lose to a great extent their natural elasticity. Thus, the over-distension of the female urethra has often produced incurable incontinence arising from this cause—which is one instance only among others which might be adduced. The distending instrument I employ is composed of two parallel rods, which can be slowly separated by means of a screw in the handle, through the action of two levers which exist between them. In this manner the rods pass gradually and successively through all the numbers of the catheter scale, from 5 to 16 or 18, in obedience to the operator's action on the screw, and an index there marks the rate of movement for his information. When open, the two blades produce the figure of a very elongated spindle, the centre of which corresponds with the site of the stricture in the urethra. This position is easily ensured by means of the graduated scale marked on the stem of the instrument, and a small clamp which slides upon it. If the stricture is five inches from the orifice, this clamp is placed against the figure 5 on the stem, and the instrument is passed until the clamp acts against the external meatus. It is to be carefully maintained in this situation while the distension takes place.

Having so placed the instrument, nothing would be easier than to turn the screw rapidly, and at once raise the calibre of the strictured portion to the required size say 16 or 18. This would effect its rupture, and is precisely that which I wish to avoid. I occupy from seven to ten minutes in slowly distending the part up to that calibre, and therefore prefer to give the patient the benefit of chloroform. Hence I *over-distend as much as possible, and rupture as little as possible.* The tightest fibres of all alone get ruptured ; those which are less rigid yield to the distending force. The more I can accomplish the latter object, the more I avail myself of the principles just alluded to, and deprive the tissues of their elasticity. The more I accomplish by rupture, the more of a wound I produce, and consequently the more fear there is of rapidly returning contraction. Having reached the limit intended, the screw is slowly turned back until the index marks about No. 10, and the instrument is withdrawn. The instrument is designedly not entirely closed. A large gum catheter is passed, and tied in for twenty-four hours, a plan which appears preferable, although there is no more occasion to do this than there is after rupture, under which circumstances Mr. Holt dispenses with the catheter altogether. I pass no instrument for two days ; then a full-sized bougie every day or two, prolonging the intervals, and teaching the patient to do it himself occasionally afterwards.

A few words on the cases in which this proceeding is not applicable, or rather in which other treatment appears to me preferable. For an old and non-dilatable stricture within two or three inches of the meatus, I prefer internal urethrotomy. It is perfectly safe and easy if performed in that situation. Dilatation and rupture, in my experience, are both inadequate to produce any lasting benefit. I have heard of my mode being tried at the anterior part of the canal. I should not have advised it. The spongy body here is so much less extensible, so little porous, and so greatly fibrous, as compared with its character where it constitutes the bulb, that but small room exists for extension. Hence in part the futility of ordinary dilatation as well as of over-distension in this situation.

Again, if a urethra is narrowed at several points—an unusual condition, but nevertheless one which is occasionally met with.—I should prefer to adopt Mr. Holt's plan of rupture as, on the whole, the most certain to hit all the points of narrowing.

There remain then all the strictures, forming a great majority which exist at from four to six inches from the meatus. For these, I may say, in one word, that, having employed all the mechanical methods of dealing with them that rest on sufficiently sound principles or on sufficiently good authority to merit trial, I know of none that has afforded me such good results, both in regard of the immediate object and of the enduring result, as the method I have endeavoured to delineate in this paper.

I could easily append, as illustrations of these remarks, numerous histories of patients so treated both in private and as hospital patients were it desirable, but it appears to me to be unnecessary.

I have applied this proceeding only for the most intractable cases which have come under my notice, having preferred always to employ ordinary dilatation (usually by tying in the gum catheter) when it was possible to accomplish my object by that safe and simple process.—*Lancet, June* 16, 1866. *p.* 655.

46.—CASE OF STRICTURE OF THE URETHRA: RETENTION OF URINE: PUNCTURE OF THE BLADDER PER RECTUM; PYÆMIA; RECOVERY.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.

[The patient was a labourer aged 34, admitted into Guy's Hospital on Oct. 11. He had gonorrhœa twelve years ago, but it was not followed by symptoms of stricture. Eight years since, however, retention of urine followed an injury in the perineum from the pommel of a saddle. The urine which was then drawn off was bloody. Four years ago he had complete retention of urine, for which a No. 1 gum-elastic was passed; and he remained under treatment until the stricture admitted a No. 10. Since then the stream of urine has been gradually diminishing, and complete retention existed when he was brought into the hospital.]

On admission, he was put into a warm bath, and two grains of opium were administered. Silver and gum-elastic catheters were then tried, but failed to traverse the stricture. Warm oil was injected into the urethra, and a fine conical wax bougie tried, but without success. As the man was now much distressed, the bladder was punctured per rectum, and a large quantity of dark-coloured, highly ammoniacal urine drawn off. Half a grain of opium was administered shortly afterwards. He was much relieved by the operation.

Oct. 12th, (second day). There has been much bleeding from the wound in the rectum, and a mixture of urine and blood comes both through and by the side of the canula. A bag of ice was ordered to be applied to the pubes, and another to the perineum. To take tincture of hyoscyamus and antimonial wine, of each twenty minims, in an ounce of acetate of ammonia mixture, every four hours; also, half a grain of opium pill three times a day.

13th (third day). Still a good deal of bleeding. After examining the patient per rectum, Mr. Hilton ascertained that the instrument had been thrust obliquely through the posterior part of the prostate gland, and traversed the prostatic portion of the urethra. There is considerable constitutional disturbance; hot skin; quick, small, and somewhat wiry pulse; anxious expression of countenance; dry, brown tongue; some tenderness on pressure over the pubic part of the abdomen. Ice to be continued; also the mixture.

16th (sixth day). Has remained in much the same state for the last three days. Blood is still mingled with the urine, though in less quantity, and there is now much pus with it. Still much tenderness just above the pubes, and some tendency to vomiting. As the bowels have not been open for some days, he is to have fifteen grains of calomel-and-colocynth pill immediately, and an enema if necessary; the opium pills to be continued after the bowels have been acted upon. Mr. Hilton tried to pass an elastic No. 4 through the urethra; but although the catheter went in a long way, he doubted if it had passed into the bladder, so a plug was placed within the rectum canula, in order to see, when the bladder became distended, if any urine would pass through the catheter. During the evening the man had severe rigors. Ordered brandy (three ounces) and soda-water.

17th (seventh day). As no urine has passed from the catheter—only a clot of blood,—a fine stilette was passed into the catheter to clear out any clots; and at about two-thirds its length it met with an obstruction, as if the catheter were bent abruptly upon itself. Urine flows both through and by the side of the rectum-canula; it contains much pus, probably the result of suppuration between the bladder and pubes, but there is scarcely any blood in the urine.

18th (eighth day). The patient has had repeated rigors to-day, and is dangerously ill. Catheter and canula both removed. Opium to be discontinued; six ounces of wine ordered.

20th (tenth day). Much better; slept pretty well last night. The urine passes readily from the rectum.

22nd (twelfth day.) Small abscess opened in the perineum: half an ounce of pus evacuated. A few drops of urine subsequently passed from the penis. He passes urine by the rectum after retaining it in his bladder for about three hours.

30th (twentieth day.) About one ounce of urine passes by urethra at each effort of defecation.

Nov. 9th (thirtieth day.) Urine has passed twice exclusively by urethra; stream small.

11th (thirty-second day.) He has had much sore throat, with difficulty of swallowing, for some days. A considerable abscess is forming in the median line of the neck, and pointing anteriorly. In the evening it is opened.

12th (thirty-third day.) The throat is no longer sore, and he feels much better. Ordered to be up a little every day.

20th (forty-first day). Mr. Hilton tried to pass an elastic No. 3, but only succeeded in getting it within the stricture, and into the prostatic portion of the urethra. It was retained an hour or two, and then withdrawn. Apparently it induced a spasmodic state of the stricture, for he could not pass any urine through the urethra for some hours afterwards. He occasionally passes a little urine by the rectum.

22nd (forty-third day.) No. 5 catheter passed easily into the bladder to-day, and the urine came away through it.

Dec. 2nd (fifty-third day.) The urethra has been gradually dilated, so that a No. 9 catheter passes easily. Another abscess, preceded by an attack of sore-throat, has formed in exactly the same position in the neck as the former one; it opened spontaneously to-day. Ordered one ounce of quinine mixture three times a day.

5th (fifty-sixth day.) Passed an elastic No. 9 himself in the presence of the dresser. At his own request he was allowed to leave, with orders to pass the catheter himself every day for some time to come, and to re-present himself at the hospital if he experienced any difficulty.

18th (sixty-ninth day). No urine by the rectum. He passes a No. 9 catheter when he wishes to void urine. Health good. He is doing well.

Observations.—Twelve years ago he had gonorrhœa, and no symptoms of stricture of his urethra followed; yet with this same malady a patient does not always so lightly escape. Now, why should one man have a stricture after gonorrhœa and another not? I have a drawing here that represents the urethra divided longitudinally, and laid open, so as to expose its mucous surface. The original preparation was taken from the body of a man, who died suddenly of some internal disease, whilst suffering from acute gonorrhœa. The gonorrhœal inflammation, you will see, occupies the anterior third or half of the urethra, whilst stricture usually occurs in the bulbous or membranous portion—that is, altogether behind the portion attacked in acute gonorrhœa. The interpretation of this may be found primarily in the fact that the acute inflammation, through injudicious local treatment (as by the oftentimes adopted but stupid method of placing a piece of lint or some cotton wool upon the mouth of the urethra, in order to prevent the escape of the discharge, by which plan no doubt the shirt is kept clean, but the retention of the gonorrhœal fluid within the urethra ensures not only the intensification but the extension backwards of the inflammatory condition; or by the injudicious employment of strong astringent, irritating injections, catheterism, &c.) or through indiscretions on the part of the patient, is made a chronic affair. The chronic inflammation travels back and reaches the equally vascular, more loose, more muscular, and more sensitive parts of the urethra—the bulbous and membranous portions. Inflammatory lymph is then effused into the submucous tissue, which lymph more readily encroaches upon the calibre of the canal in this situation, because the urethral wall is not at those parts so closely united to the surrounding tissues. The urine then passes with difficulty, and the additional effort made to expel it excites the canal at the strictured part and behind it. A catheter is used, perhaps wounds the mucous membrane, and a permanent stricture may be the result of this complication of troubles. An attack of gonorrhœa, unless very acute or very enduring, will give rise to no stricture, provided it be treated properly, and the patient take care of himself, committing no indiscretion as regards diet, women, or mode of living generally. For the gonorrhœal inflammation is subservient to the general law, that all local inflammations are modified by the state of the general health, whether the inflammation be of the urethra, or of any other part of the body.

The explanation of the acute inflammation becoming at first localized within the anterior part of the canal is probably due to the manner in which the local disease is contracted. It may result from the forced contact of the morbid fluid with the meatus, and thence its extension inwards. There is another probable explanation—namely, that during coitus, and immediately after the seminal emission, a vacuum is produced in the fossa navicularis of the urethra; and some of the morbid fluid from the vagina of the female is forced by atmospheric pressure through the meatus, and is thus brought into direct contact with that portion of the urethra which observation declares to be the early seat of the gonorrhœal inflammation. Otherwise than by some such manner, it is unintelligible how the gonorrhœal fluid can enter the male organ. I believe that gonorrhœa is produced by this unhealthy fluid being allowed to remain during some time upon the os urethræ or within the fossa navicularis, and so inoculating the mucous membrane. Practically, if this be true, it points to the advantage likely to accrue to the male by his passing his urine forcibly soon after connexion, in order to wash out the gonorrhœal fluid.

But to return to our case. Eight years ago the patient received an injury in the perineum from the pommel of a saddle. We cannot tell positively if the urethra was then wounded; but it is a fair supposition that it suffered some amount of damage, such as contusion or laceration opposite the perineum; for from that moment he had retention of urine, so that a catheter was needed to remove from his bladder a large quantity of bloody urine. He forgets whether blood flowed from the meatus directly after the accident, which would certainly indicate laceration of the urethra; or whether it came mixed with urine, &c. At any rate, ever since the accident he has suffered from stricture; so I conclude the urethra was then injured, and a true traumatic stricture resulted.

As a digression, we will see what should be done in case any of you are called to a man who has suffered an injury in the perineum, such as might be produced by a fall upon the pommel of his saddle. First of all, do not ask the man to pass urine, because if his urethra be lacerated, his straining efforts at micturition may cause extravasation of urine at the seat of injury; but pass a catheter, and draw off his urine. Should the instrument go in easily, and not any blood appear, you may be tolerably sure that the urethra has suffered no material damage. If you cannot pass the catheter, leave him alone for a while, and give him opium. The purpose intended by the administration of this drug I will presently explain. But if you are certain from the appearance of blood upon his shirt and blood escaping from his penis, without an external wound to account for it, that the man has laceration of the urethra, then you must if possible, and without delay, pass a catheter of a large size—one that enters freely the meatus; for a large catheter is likely to push aside the torn pieces of the urethra, and place them again in their proper position, whilst a small one is more likely to enter the wound of the urethra. The catheter is to be passed for two purposes—firstly, to relieve the man of his retention of urine; secondly, to prevent the urine passing from the lacerated urethra, and extravasating into the adjoining tissues. Now, if you cannot pass the catheter, you must accomplish the two purposes you have in view by some other means, which you do by cutting into the middle line of the perineum behind the seat of injury, and opening the urethra, and then passing a female or gum-elastic catheter through the perineal opening into the bladder. If from the great stoutness of the patient, or from some other cause, you deem that great difficulty, and possibly failure, may attend this operation, you must puncture the bladder by the rectum, and then after a while you will probably be able to introduce a catheter through the urethra ; for the bar to the catheter introduction may have been caused by swelling, the result of the local injury, which will have subsided.

Four years ago this man had complete retention : this was but part and parcel of his malady. The wound in the urethra, which was caused eight years ago, was healed by the effusion of lymph at the part ; this cicatricial lymph afterwards contracted (as is the case with lymph in most cicatrices when frequently disturbed by either normal or abnormal function), the calibre of the canal was encroached upon, and at length (perhaps from some indiscretion at the time upon his part) complete retention was caused. A small catheter was, however, introduced ; and the stricture was cured by dilatation. Had he then continued to pass the catheter once or twice a week, he would probably have remained all right ; but he neglected himself, and consulted some quack, who, as he said, had during six months been "working away at his passage" with a No. 1 catheter without success. The cicatrix had again contracted, till at last, two days before his admission here, he had complete retention of urine.

In dealing with ordinary cases of stricture, whether from injury or from chronic inflammation, my advice is that you do not adopt *vi et armis* the heroic line in their treatment, but pursue the plan of slow dilatation by catheters of gradually increasing size. You must remember that many of the cases you see in this hospital are exceptional ones—much more severe than those you will hereafter meet with in private practice. You may, perhaps, in the course of a large practice and a long life, meet with one or two as aggravated as many of those admitted here, in which it is impossible to introduce a catheter. Your patients will come to you as soon as they find the stream of urine getting less, and you will be able to arrest the course of the complaint and to effect a cure by dilatation. Some surgeons in the North declare that they can pass an instrument through any stricture. On the other hand, surgeons in London meet with cases where they fail to pass a catheter. So that it comes to this : either the London surgeons are inferior in manipulative dexterity, or the cases they treat are more severe. Dr. Frederick Hughes Hewitt, formerly a distingushed student at Guy's, who spent many months in Edinburgh a few years ago, informed me that he never saw there cases of stricture so severe and so complicated as many of those he has seen in Guy's. What may be the explanation of this difference I cannot say.

The patient, then, came to us with complete retention, and was treated first with opium. Upon what rule? Because, usually, spasm of the urethral muscles is superadded to an inflamed state of the urethra in cases of stricture; and it is the muscular spasm which at length completes the retention. You put the man into a warm bath, give opium, and administer chloroform; all these agents annihilate muscular spasm, and usually render it possible for you to pass a catheter, or for the patient to micturate. In our patient the cause of the stricture was persistent, and had been persistently increasing for four years before his admission; this showed that it was not spasm alone, but a continually increasing thickening of the walls of the urethra, although in this case limited to the actual seat of stricture. And I may add that the strictured part was very sensitive and painful to the touch. Very little surrounding thickening could be felt in this instance, and the case was one in which false passages can be easily made, as is very frequently done by using force in catheterism. Had the patient come to us before his condition was so extreme, would it have been possible to prevent the complete retention? Certainly. I tell you most distinctly and emphatically that many cases of stricture may be cured by medicinal treatment and improving the general health. With a view to the absorption of the chronic inflammatory thickening, I should have given him small doses of mercury and chalk twice a day, adding Dover's powder in order to quell muscular spasm. Further, the patient would have been kept in the recumbent position (this advice applies to all prostatic and urethral affections), by which means you displace by gravitation the congestion of bloodvessels.

But, after the warm bath and the opium, it was still found impossible to pass a catheter, so (in my absence) puncture of the bladder by the rectum was performed. Now, how is this operation best to be done? First, put the patient as systematically in the proper position for the operation as if you were going to cut him for stone; then pass your forefinger into the rectum, in order to feel and examine well the prostate. When in going round my wards I have any case where a digital examination of the rectum will assist the diagnosis, I make it a point to invite my dressers and some others of the class to do the same, for the purpose of enabling them to familiarize their finger with the healthy or unhealthy state of the prostate. First, one shrugs his shoulders and turns away; his fingers are too delicate for such a dirty place. Then another has gloves on or his hands in his pockets, and thinks it a nasty thing to do; and in the end only a few are found to follow my example. This is not right, for the only opportunity by which you can gain experience in such matters is whilst at the hospital, and I beseech you to do it as often as possible. It is a bit of experience which will be worth a great deal to you hereafter. The truth of my words some of you will perhaps realise, when some rich old gentleman, willing to pay anything for really good attendance, is getting fidgety, because you, not knowing the cause of his malady, cannot cure him. Well, then, pray do examine the prostate, in order to familiarize the finger with its form, proper configuration, and normal degree of resistance at different periods of life, and to know if it be softened or hardened, &c. Before performing the operation of puncturing the bladder by the rectum, it is especially necessary to be able to feel the prostate with precision. You may sometimes, perhaps generally, feel the lower part of the distended bladder projecting downwards from behind and above the prostate; but not always so, for sometimes the bladder is drawn or dragged upwards into the abdomen, and the fuller the bladder in these latter cases the less can you generally feel of it by the rectum. But you *can* always in these same cases feel the distended bladder by pressing it down from the front of the abdomen into the pelvis with one hand, and by placing the finger of the other hand within the rectum. Why do these differences in the position of the distended bladder obtain in different persons? A short urachus, which acts as a suspensory ligament of the bladder to the umbilicus, will retain the organ forwards and upwards; perhaps, also, other causes may tend to hold it within the abdomen rather than in the pelvis.

In performing the operation, let there be no hurry: feel the prostate well; keep your eye fixed on the pubes or the umbilicus, so as to guide your finger exactly in the median line, and if possible push your finger beyond the prostate. These little points all help to make the operation successful. If you deviate from the median line, you are sure to do mischief, by wounding the vas deferens, or puncturing an artery or veins. These are amongst the most common casualties associated with this operation. If you do not feel the bladder, be sure it is one of those cases where it is raised into the abdomen; get an assistant to push it down, and to keep his hand there until you feel the bladder and have punctured it. Upon the introduction of the trocar and canula the urine comes away.

On the day following the operation much blood escaped from the wound in the rectum, so I ordered a bag of ice to be applied over the pubes and another to the perineum. Next day, the hemorrhage still continuing, although lessened, I could not help thinking something had gone wrong. I therefore examined the patient per rectum, and found that the instrument had been thrust by the operator obliquely through the prostate gland and the posterior part of the prostatic portion of the urethra, and most likely wounded the front wall of the bladder behind the pubes. The man was suffering from much constitutional disturbance, of which he had all the usual symptoms; further, he had great tenderness over the lower part of the abdomen, and a tendency to vomiting, showing inflammation either within or near the bladder—all bad indications, prospectively at any rate. The projecting end of the trocar only had probably just pierced the front wall of the bladder sufficiently to allow of slight extravasation of urine behind the pubes, and this, I apprehend, was the cause of both the tenderness there upon pressure, and of the constitutional disturbance. Now there is occasionally, but very rarely, an artery or veins near the median line of the bladder behind the prostate; so that if you puncture the bladder (not the prostate) with precision in that line, you might wound a bloodvessel there or in the walls of the rectum. Such a casualty is unavoidable. You may open a small vessel at the part, but it will not bleed much. I have no doubt I did in one case puncture the vas deferens by not keeping the instrument precisely in the median line. The man subsequently had inflammation in the testicle of that side, and finally the testicle became atrophied.

Seeing that the canula (in the case now occupying our attention) had certainly gone elsewhere than merely into the cavity of the bladder, on the sixth day I thought it best to try and pass a catheter, hoping to dispense with the further presence of the canula within the rectum and bladder. For this purpose I endeavoured to pass a No. 4 gum-elastic catheter. It went as far as the canula within the prostate, and then turned; so that I could not tell where it had gone. I was anxious to know if it had entered the bladder; and, to prove or disprove the point, I plugged the canula, so as to prevent the urine from passing by the rectum. On the next day no urine had come by the catheter, which, as it turned out subsequently, was bent on itself, and had not entered the bladder at all.

On the eighth day the man was obviously going down hill. The canula had transfixed the prostate obliquely; and this alone was a source of great irritation. It was besides, impossible to tell with precision where the point of the trocar had gone; and, in withdrawing the canula, there was some risk of retention or of extravasation of urine. The canula has two purposes to accomplish: to relieve the retention, and to relieve the stricture also, which latter purpose it effects by allowing the strictured part to be at perfect rest, with no urine pressing against it or passing over it; and, as a rule, you do not withdraw the canula until the man can pass urine by the urethra, or a catheter can be introduced by the urethra into the bladder. This, however, was not a simple case, and we were not able to wait until the urine came by the penis; so, although by withdrawing the canula we ran the chance of permitting some mischief to occur, yet it was a choice of evils, for he was tending to death, and he must submit to his risk. Two days subsequently a marked improvement in his condition was perceptible, showing that we were right in withdrawing the instrument, although we acted contrary to the rule that the canula is not to be withdrawn until the patient can pass urine, or the surgeon introduce a catheter. The hole through the prostate into the bladder was probably valvular; hence the fact that he could retain his urine for about three hours, after which the rectum received the contents of the bladder.

Four days after the removal of the canula an abscess was detected in the perineum, the effect probably of the local irritation excited by the stricture, the attempts at catheterism, &c. When the abscess was opened about half an ounce of pus escaped by the hole in the perineum. Urine began directly afterwards to flow from the urethra. I think, then, the fair supposition is, that the abscess, by the pressure upon the urethra it exercised before it was opened, prevented the passage of water through the urethra.

The man at this time had an abscess in the front median line of his neck. It might have been caused by some local injury; but the fact of its being so symmetrically in the middle line favours the supposition of its having its origin in some constitutional or generally diffused affection—a blood-poison, as pyæmia. An abscess in the median line, which affects consequently both sides alike, is usually caused by a constitutional disorder. A carbuncle is usually in the median line, as the nape of the neck, the back, or abdomen; or it may be equally developed on both sides of the body. Boils, cutaneous eruptions, and scrofulous glands (each arising from a generally-diffused cause) are also situate either in the median line, or are bilaterally symmetrical. Pyæmia is less controlled by this rule than any other blood-poison, for it does not at all necessarily show symmetry in its secondary suppurations. Pyæmic superficial abscesses are probably obedient to, and subserve, the rule of previous deterioration of structure. When a man has an impaired state of health—and it is always very much impaired in pyæmia—any previously deteriorated part of his body is the most likely to suffer. The internal suppurations, however, seem to follow the formula of "local symptoms being symmetrical in general blood affections." The fact of abscesses occurring in the liver, which is not symmetrically placed in the body, does not invalidate this position; for the lungs and liver may both be regarded as central organs, seeing that all the blood of the system goes through the former, and all the portal circulation traverse the latter. There must have been some determining cause for this large abscess in the median line, so far removed from the seat of his bladder. We must not shelter ourselves under the obscurity of "coincidence" or "anomalous," but endeavour to draw aside the curtain that at present veils the true cause from our view. Think over this matter ; ask yourselves, what can be the explanation? Reason it out for yourselves. You may fail to discover the interpretation, but you will have rightly exercised your high prerogative of reason. —*Lancet, July* 28, 1866, *p.* 87.

47.—ON THE MEANS TO BE ADOPTED FOR ESTABLISHING A COMMUNICATION BETWEEN THE BLADDER AND THE EXTERIOR OF THE BODY, WHEN THE URETHRA HAS BECOME IMPERMEABLE.

By Edward Cock, Esq., Senior Surgeon to Guy's Hospital.

In this short communication I have no intention of alluding to the treatment of ordinary stricture ; nor shall I yield to the temptation of criticising the various methods and appliances which, within the last few years, have been either invented or resuscitated for the purpose of restoring a narrowed urethra to its normal condition. Some ingenious mechanical contrivances cut, some tear, some burst, some are supposed to dilate with marvellous rapidity. My objection to their use is founded on a forty years' experience, which has taught me that such cutting, tearing, bursting, or rapid dilatation is often exceedingly mischievous in its effects, and fails in establishing a permanent cure, and that the object in view may be accomplished by much milder, surer, and safer means.

The purport of this paper is not to show how a strictured urethra may be treated, but how the bladder may be got at when the canal has become positively and absolutely impermeable to instruments of every description.

Some years ago I sent to the Medico-Chururgical Society a paper, which was read and published in the "Transactions," detailing my experience in the operation of puncturing the bladder through the rectum.

The object which I then had in view was to prove, by the test of experience in forty cases, that this operation is safe, easy of accomplishment, and without danger as to its consequences, and that in cases of retention which resist ordinary treatment it is greatly to be preferred to long-continued attempts at catheterism, which, whether successful or not, must be infinitely more injurious to the urinary organs than the simple and almost painless operation of tapping.

I considered that the benefit of the operation consists not merely in the immediate relief given to the patient, but also in the opportunity which it affords, by the retention of the canula in the bladder through an indefinite period, of diverting the flow of urine from its ordinary channel, and thus giving quiet, freedom from pain, and the natural means of restoration to the maimed, irritable, or diseased urethra.

From this point of view I conceived that the bladder might be tapped with advantage in cases of obstinate stricture, in which retention of urine did not actually exist.

I wished to rescue the operation from the obloquy which had been cast upon it by every surgical writer, as a desperate kill or cure remedy, only to be used when every other means had failed; and to restore it to its legitimate place in the category of surgical means and appliances.

It is gratifying to know that since the publication of my paper in the "Transactions," the bladder has been tapped by many surgeons, and that even some of the warmest opponents to the operation have since received it as a very available friend in need.

Still, however useful and available may be the means of relieving the bladder by tapping through the rectum, this operation does not fulfil every necessity that may arise; for, to ensure a successful result, certain conditions are required, which do not always exist. An empty, habitually contracted bladder, and a very large prostate, are almost fatal objections to the operation.

A class of cases not unfrequently come under our observation, where the bladder must be got at somehow to save the life of the patient, and where yet it cannot be reached, either by ordinary catheterism through the urethra, or by tapping above the pubes or through the rectum.

We tap the bladder because it is distended and we cannot pass a catheter. But there are other conditions of the bladder more chronic, yet not less fatal, than acute retention. Every surgeon must be familiar with cases where stricture has existed for a number of years; where the urethra has become permanently obstructed, or destroyed by the constant pressure of urine from behind, and by reiterated attempts, generally fruitless, to introduce an instrument; where extravasation into the perineum has again and again taken place, causing repeated abscesses and their consequences, the formation of urinary sinuses and fistulæ, until the normal textures of the perineum become obliterated, and are replaced by an endurated, gristly structure; where the bladder has become thickened, and contracted by the constant action of its muscular coat until little or no cavity is left; and where the urine is constantly distilling by drops, either through the urethra or through one or several fistulous openings, which dot the surface of the perineum, penetrate through the indurated scrotum, and even find their way to the nates below, and the region of the pubes above. If unrelieved, these cases invariably terminate fatally. The patient slowly sinks under his constant efforts to micturate, and the unceasing fever and constitutional irritation produced by the passage of the urine through abnormal channels, and the frequent formation of fresh abscesses. Finally, the ureters and pelves of the kidneys become dilated ; renal disease is set up ; and the patient at length dies with symptoms of blood-poisoning by urea.

To remedy this complication of evils—viz., an impermeable urethra : a disorganised perineum, of which all, or nearly all, the anatomical features have disappeared ; fistulous sinuses communicating with the urethra, and distilling urine ; recurrent abscesses from the irritation of the extravasated fluid ; broken down health from diseased bladder, the forerunner of renal disorganisation ;—I say, to give the patient a chance of life or restoration, it is necessary to establish a free communication between the bladder and the exterior.

The natural passage through the urethra is not available, as the channel has long been either rendered impermeable or altogether destroyed. The sound and catheter are therefore useless ; while the contracted condition of the bladder forbids the operation of puncture behind the prostate.

The surgical means adopted in these miserable cases should have a twofold object in view—an immediate and a more distant result. The one object is the prompt establishment of a free communication with the bladder behind the obstruction. The second and more remote is the restoration of the urethra to its normal condition, so far as this may be possible.

The operation which is recommended and described in most books of surgery proposes to combine both these objects—the relief of the bladder, and the cure of the stricture. It consists simply in effecting a union between the two permeable portions of the urethra, by cutting down upon and dividing the intermediate strictured or obstructed portion. "The urethra is opened anterior to the stricture on a staff carried down to the spot. The stricture is then to be traced backwards, and divided until the canal behind it is reached and laid open. A flexible catheter is then to be introduced through the urethra into the bladder, a portion of the catheter representing the splice between the two permeable portions of the canal. The wound fills up and unites over the catheter, and the cure is then complete."

The theory is plausible and good, could it be carried out, but the practice is most difficult, tedious, and severe ; often fatal, and always unsatisfactory as regards the ulterior result. Even if all the tissues of the perineum were in a healthy normal state it would be most difficult, often impossible, to lay open a stricture which lies at the bottom of the wound, and which has long been impermeable to the smallest instrument, and can neither be seen nor felt by the operator. I have frequently seen this operation performed, or rather attempted; and I doubt whether in any one instance the original intentions of the surgeon were carried out. In several instances death followed as a consequence of the operation, and a *post-mortem* examination revealed that, although all the structures of the perineum had been most unnecessarily incised and damaged above, below, before, behind, and round about the stricture, it was the stricture alone which had escaped untouched and without mutilation, while the catheter had been carried out of the canal above the stricture, and reinserted below. This very circumstance is fatal to the prospects of a permanent cure, for the splice, not consisting of restored urethral tissue lined by mucous membrane, but being a mere factitious canal, formed by granulations around the catheter, will begin to contract as soon as the use of the instrument is abandoned, and will, at no very distant period, become again impermeable. I need scarcely say how all the difficulties I have described must be multiplied when the perineum is swollen, indurated and perforated by sinuses, when every anatomical guide that could be appreciated by sight or touch has disappeared, and when it is impossible to ascertain how far backwards the obstructed and diseased portion of the urethra extends; and yet these are precisely the cases in which surgical interference is necessary.

For many years I have adopted another operation, which is at once easier, simpler, and much more successful; for the objects aimed at in the operation I am about to describe can be accomplished, while I doubt if those of the other have ever been fulfilled.

However complicated may be the derangement of the perineum, and however extensive the obstruction of the urethra, one portion of the canal behind the stricture is always healthy, and often dilated, and is accessible to the knife of the surgeon. I mean that portion of the urethra which emerges from the apex of the prostate—a part which is never the subject of stricture, and whose exact anatomical position may be brought under the recognition of the finger of the operator.

Thus, when we cannot introduce a catheter by the ordinary method, and even when we cannot tap the bladder through the rectum, it still remains to us to tap the urethra as it emerges from the prostate, and thus to effect the desired communication.

There are considerable advantages attending this operation. The bladder is reached without any unnecessary mutilation of the perineum. The communication is effected in nearly a straight line from the exterior to the cavity of the viscus ; so that the canula, which is inserted and retained, can be removed whenever necessary, and can be easily replaced, The functions of the entire urethra are suspended, and may be kept in abeyance for an unlimited period. The urine no longer finds its way abnormally through the stricture and sinuses of the perineum. The tissues are no longer subjected to constant irritation from infiltration. The constitutional symptoms are relieved, and time and opportunity are given for the removal, by absorption, of those adventitious deposits which obstructed the urethra, indurated the perineum, and rendered the introduction of an instrument impossible. The pressure on the kidneys is removed, and, if expedient, the bladder may be readily washed out, until its lining membrane assumes a healthy character. The strictured and damaged portion of the urethra, being no longer subjected to the constant pressure of urine from behind. may probably so far recover itself as to allow of restoration by the ordinary means of dilatation ; or, should the canal have become permanently obliterated, the patient still retains the means of emptying his bladder through the artificial opening without difficulty or distress, and at very moderate inconvenience to himself.

This operation (which for the sake of distinction), I may call "tapping the urethra at the apex of the prostate, unassisted by a guide staff," is alluded to in some books on surgery, but does not appear to have found much practical favour. I have performed it many times, but have seldom seen it attempted by others. Indeed, I do not think that the mode of procedure recommended in surgical works and manuals is likely to lead to a successful result ; for it is made to depend on a slow and protracted anatomical dissection, through structures where, almost invariably, all anatomy has been destroyed, or if present, cannot be brought under the eye of the operator.

My experience has suggested the following method of reaching the urethra; and by adopting it I have never failed to accomplish the object in view.

The only instruments required are, a *broad* double-edged knife, with a very sharp point ; a large silver probe-pointed director, with a handle; and a canula, or a female catheter modified so that it can be retained in the bladder.

The patient is to be placed in the usual position for lithotomy ; and it is of the utmost importance that the body and pelvis should be straight, so that the median line may be accurately preserved. The left forefinger of the operator is then introduced into the rectum, the bearings of the prostate are carefully examined and ascertained, and the tip of the finger is lodged at the apex of the gland. The knife is then plunged steadily but boldly into the median line of the perineum, and carried on in a direction towards the tip of the left forefinger, which lies in the rectum. At the same time, by an upward and downward movement, the vertical incision may be carried in the median line to any extent that is considered desirable. The lower extremity of the wound should come to within about half an inch of the anus.

The knife should never be withdrawn in its progress toward the apex of the prostate; but its onward course must be steadily maintained, until its point can be felt in close proximity to the tip of the left forefinger. When the operator has fully assured himself as to the relative positions of his finger, the apex of the prostate, and the point of his knife, the latter is to be advanced with a motion somewhat obliquely either to the right or the left, and it can hardly fail to pierce the urethra.

If, in this step of the operation, the anterior extremity of the prostate should be somewhat incised, it is a matter of no consequence.

In this operation it is of the utmost importance that the knife be not removed from the wound, and that no deviation be made from its original direction, until the object is accomplished. If the knife be prematurely removed, it will, probably, when reinserted, make a fresh incision, and complicate the desired result. It will be seen that the wound, when completed, represents a triangle; the base being the external vertical incision through the perineum, while the apex, and consequently the point of the knife, impinges on the apex of the prostate. This shape of the wound facilitates the next step of the operation.

The knife is now withdrawn, but the left forefinger is still retained in the rectum. The probe-pointed director is carried through the wound, and, guided by the left forefinger, enters the urethra and is passed into the bladder. The finger is now withdrawn from the rectum; the left hand grasps the director, and along the groove of this instrument the canula is slid until it enters the bladder.

The operation is now complete, and it only remains to secure the canula in its place with four pieces of tape, which are fastened to a girth round the loins. There will probably be no escape of urine until the stilette is removed from the catheter.

A direct communication with the bladder has now been obtained, and the relief to the patient will be immediate. Unless the kidneys have become irremediably disorganised, we may confidently anticipate a favourable result; and the restoration of the urinary organs will be more or less complete, in proportion as the obstructed portion of the urethra is more or less amenable to the ordinary judicious treatment of stricture. The canula may generally be retained in the bladder for a few days, and if the state of the urine renders ablution necessary, the viscus may be frequently washed out. The canula may then be removed, cleaned, and reintroduced. A flexible catheter is sometimes more desirable and congenial to the feelings of the patient than a metallic canula.

[To keep the artificial passage in a permeable state, it is generally necessary to pass a flexible bougie through the opening occasionally, and to retain it *in situ* for a few hours. The patient very soon learns to do this for himself.]--*Guy's Hospital Reports, Vol. XII., p.* 267.

48.—ON LITHOTRITY.

By T. Pridgin Teale, Jun., Esq., M.A., Surgeon to the Leeds General Infirmary.

[After relating and comparing a number of cases of lithotomy and lithotrity, Mr. Teale offers the following remarks.]

1. Where there is no impediment to the passage of the lithotrite either in the urethra or prostate, and where the bladder can retain sufficient urine to allow the lithotrite to be worked with safety, I believe that most calculi which can be grasped and broken through admit of being safely dealt with by lithotrity.

2. Where no injury has been done to the bladder in using the lithotrite, the stone when broken into fragments is less distressing to the bladder than in its unbroken state. Should this fact, observed by Sir Benjamin Brodie and others, and confirmed by several cases of my own, prove to be generally true, it will still further reduce the question between lithotomy and lithotrity to one of the possibility of breaking the stone by the lithotrite.

3. If a fragment of stone can pass the neck of the bladder and the prostatic portion of the urethra, it can generally traverse the remainder of the urethra without difficulty. I only remember one instance in these cases in which a fragment had to be removed from the urethra, and this was arrested in the glans by the narrow meatus.

4. In several cases, especially in the latter ones, very large fragments have been passed. The passage of large fragments is a good symptom—a sign that the neck of the bladder and the prostate have suffered but little from the use of the lithotrite. We often find that only a few fragments are passed during the first twenty-four hours after lithotrity; then small fragments appear in greater number, and lastly the larger ones. In such instances the prostate has become swollen, and thus opposes a temporary barrier to the escape of the stone; but as the swelling

subsides the freedom of the passage is restored, and the fragments begin to come away.

5. As to the reduced risk to life from the adoption of lithotrity, I would not venture to say a word from such a small number of cases as my own. I may, however, appeal to the experience of others. Mr. Henry Thompson, in his work on Lithotomy and Lithotrity, records the results of 1827 cases of lithotomy which he had collected from various sources. The fatal cases were 229, or 1 in 8; or if those below the age of sixteen be taken alone, there was a mortality of 1 in 17½; if those above sixteen years be taken, a mortality of 1 in 5½. Sir B. Brodie, in 115 cases of lithotrity, had a mortality of only 1 in 12½. Mr. Henry Thompson has recently published a series of 43 cases of lithotrity in succession without a death. If, therefore, the mortality in adult stone cases, which is usually 1 in 5 or 6, can be reduced to a mortality of 1 in 12½, or even to a much smaller proportion than this, there can be no question as to the value of the operation by which this result can be obtained.

6. Though my cases are not numerous enough to give any reliable data about the fatality of the operation, there are other points in which they can be favourably compared with lithotomy. One patient was cured in one operation, and left the hospital and resumed his work in ten days. A second underwent two operations in the hospital, left at the end of a week, and resumed his work; had a third sitting as an out-patient, and was well in less than a month. A third was under my care in lodgings for a fortnight, where he underwent three operations. He then returned home, and resumed his employment; and, after three more sittings at my own house—after which he returned home by railway,—was well in a month. A fourth remained in lodgings three days after the first sitting; after each of five subsequent sittings he returned home by train the same day, and was well in seven weeks and a half. These latter patients have not been confined to bed except for half a day after the earlier sittings.

In conclusion, let me remind you that lithotrity is an operation in which success depends upon attention to minute and delicate details. The stone must be crushed; but the bladder must not be injured. Operations too much prolonged, or repeated at too short intervals, retard progress. Therefore it is that, in order to test my own progress and to learn as much as possible by my own experience, I carefully note in these cases the date of each sitting, the number of times the lithotrite is screwed down upon the fragments, the weight of fragments passed after each sitting, and the number of days or parts of days that each patient is compelled to remain in bed.—*Lancet, Sept.* 29, 1866, *p.* 349.

49.—NEW INSTRUMENT FOR REMOVING THE SMALLER FRAGMENTS OF CALCULUS AFTER THE OPERATION OF LITHOTRITY.

By John Wood, Esq., King's College Hospital.

[Mr. Wood has invented an ingenious instrument with which he proposes to remove the detritus remaining in the bladder after the calculus has been crushed.]

It consists of a silver catheter (of which several sizes have been made), having a large oval opening on its convex surface. Within this works a stout piston-rod packed with cork. Attached to the extremity of this rod is a piece of watch-spring, which moves in the bend of the catheter and carries at the end a scoop, and in the middle three small catches, the object of these being that when the sedimentary water in the bladder is pumped out by the piston, the small fragments and particles of stone being thus brought by the suction to the opening in the catheter, they may be caught and withdrawn by the scoop and catches placed on the flexible termination of the piston.—*Medical Times and Gazette, Oct.* 13, 1866, *p.* 394.

DISEASES OF THE SKIN.

50.—ON ECZEMA.

By Dr. T. W. Belcher, M.A., Physician to the Dublin Dispensary for Skin Diseases, &c.

[The patient whose case forms the text of the following paper was three and a half years of age, and had an eczematous eruption on the scalp of several months' standing.]

I directed the hair to be cut close, and kept so by repeated use of the scissors; to keep the scalp clean, and to apply with a brush the strong tincture of iodine, which I generally use for such purposes. It is made with methylated spirit instead of with proof spirit; and in this I think it has a great advantage over the officinal preparation in any affections for which the local use of tarry preparations, or of medicines allied thereto, may be thought desirable.

The application was repeated several times, and during this period she occasionally took alterative powders of mercury with chalk.

On the 4th of April (1864), she showed symptoms of what is often called "a bad state of the system," by the breaking out of small boils on the scalp.

Cod-liver oil, in drachm doses, thrice daily, was given from this date; and on the 16th of May she was discharged cured of the skin disease, and very much improved in her general health.

I shall now give an illustration of a different form of eczema.

J. H., aged 19, by occupation a porter, was admitted to the dispensary on the 18th of April, 1865, with a (nearly) circular eczematous eruption on both arms and legs, and on the scrotum. I arrived at the conclusion that it was a case of eczema *impetiginodes*, for the following reasons :—

From some of the patches arose a serous, from others a sero-purulent exudation ; but on nearly all there was to be seen a constant formation of yellowish-brown scabs.

The quantity of the discharge was very great in this case ; I have never seen it greater in any other, and it was a source of much inconvenience as well as annoyance to the patient, who had all the while to follow his daily labour. The tincture of iodine was applied here after the manner detailed in the last case ; it was frequently repeated, and throughout the patient took the solution of the arseniate of soda (Pearson's solution) in three-minim doses, thrice daily. At first he took it in infusion of quassia ; after a while this was changed to infusion of gentian ; and finally, the vehicle used was infusion of cascarilla. The patient steadily improved, and all traces of the disease having for a short time disappeared, he was discharged, apparently cured, on the 21st of July, taking the arsenical mixture up to that day. His treatment lasted about three months. He was very well afterwards until about the middle of last month (March, 1866), when he again came to the dispensary with a very slight return of his former malady. I may return to his case in a future paper.

Eczema in a more ordinary and a more closely typical form occurs frequently on the scalp of infants during teething. On reference to my private case-book, I find that just about the same time (April, 1865), I was attending a gentleman's child, aged about five months, who was affected in this way. The tincture of iodine was tried in this case also, and when it had stopped the copious "weeping," I resorted to ointments to allay the intolerable itching which the little patient felt. I tried first of all Prof. Macnamara's "Ceratum lauro-cerasi" (see his edition of "Neligan's Medicines," p. 292). This soon lost its effect, and then I tried with more success a cerate of cold cream with chloroform, twenty minims to the ounce. Finally, I completely succeeded with thirty minims of it to the ounce. The cerate was kept in a stopped phial, and throughout the child had alterative, and occasionally purgative, medicines.

I think we may deduce some practical lessons from the consideration of eczema as brought under our notice by these cases.

The first is this: that the eczema of books and the eczema of practice are not always identical.

In my experience the purely typical disease is not so often met with as the complicated. I here take the term complication in its widest sense, as not only comprising the complication of eczema with other distinct cutaneous diseases, but also, and chiefly, the complication of the typical form with one or more of its known varieties.

In the first of the cases just detailed there was not *prima facie* any complication; but had we seen it earlier, we probably should have noticed the typical form which passed away with the acute inflammatory stage, on one part of the scalp, and the disease as it appeared on the 24th of March, 1864, on other parts.

In the second case it was clearly complicated in the sense above given. On reference to several cases recorded by me in the *Dublin Quarterly Journal* during the past year, I find in the February number (page 252) a case of scabies with eczema, and a case of eczema impetiginodes. Again, in the May number (page 495), another instance of the same kind, and also one of syphilitic eczema, which, however, we shall not now discuss, as it should be properly ranked among the "syphilides." Further, in the August number (page 254), a very remarkable case, to which I have particularly referred in my edition of "Neligan on the Skin" (page 105). Here the patient had, at the same time—1, on the right arm, a typical eczematous eruption (vesicular); 2, on the left arm an impetiginous eruption; 3, on the left cheek an erythematous blush. In a fortnight after, the diseased skin of the left arm became "rimous"—the eczema fendillé of Hardy and the French school.

I may further observe, in connexion with this subject, that greasy applications are not favourites of mine in eczematous cases. I find that they rarely do good. The local treatment which I mostly adopt is such as I have already described; where itching is troublesome, stuping with infusion or decoction of bran is of great service, and mucilage of starch, applied night and morning, in almost every case "mops up" the weeping, and fits the case for the local application of iodine. In inveterate cases where the skin is in the scaly stage, and where there is no infiltration, I repeatedly have used Hebra's tincture, or tinctura saponis viridis cum pice, with good effect, and I can confidently recommend it. Its composition and mode of application I quote from my edition of Neligan, p. 114:—"This consists of equal parts of tar, soft soap, and methylated spirit. It should be applied twice daily, suffered to dry on the skin, and washed off with soft soap or petroleum soap;" the only kinds, I may add, which ought to be used in this disease. This preparation, as its name denotes, was invented by Professor Hebra, of Vienna, and was recommended to me by Dr. McCall Anderson, of Glasgow, about two years ago.

But it is on the constitutional treatment that I place most reliance in cases of eczema. Iodide of potassium is good, but arsenic is much better ; and I never met a case yet that stayed with me long enough and was not cured.

It will take months—from three to nine, I find—to effect a cure, but it can be done, and only by combined perseverance on the part of both patient and physician. Most of the cases I have met with have been with Dr. This and Dr. That ; but, with scarcely an exception, none of the many doctors has had fair play from the patients. The patient generally gets dissatisfied when not cured at once, especially when the disease gets aggravated, as it almost always does immediately preceding the amelioration caused by continuous arsenical treatment.

Another capital error is not to continue the constitutional treatment for some months after apparent cure. This is instanced in the second case given in this paper. The patient reappears in eight months with the disease in a mild form. It should be remembered that medicines and local applications which suit one case will often prove useless in another, which to all appearance may be similar.—*Medical Press and Circular, April* 18, 1866, *p.* 391.

51.—THE CURE OF ITCH.

Let the affected parts be well washed with a strong warm solution of common soda, and, after being dried, then apply very freely paraffine; and a few repetitions will accomplish all you need. The plan is cheap as well as simple, and now paraffine is to be found in almost every cottage.—*British Medical Journal*, *Sept.* 1, 1866, *p.* 271.

52.—ON INFLAMMATION AND ABSCESS.

By F. C. Skey, Esq., F.R.S., Casualty Surgeon to St. Bartholomew's Hospital.

It is a prevailing error among the unthinking members of our profession which refers all examples of redness of vessels to inflammation. No term can be more inappropriate. The ancient definition of inflammation is founded on truth, and will hold true to the end of time : but it is rendered absurd by the practice of the modern school. In the too frequent employment of the name we forget the conditions essential to it. The arterial and capillary systems perform their healthy functions so long only as they retain the normal influence of the nerves supplying them, whether locally or generally. If the supply of nervous influence fails, the capillary system of a part loses its healthy tone, and the vessels locally dilate. At the same time they lose the power of propulsion, and there arises of necessity a local remora or arrest of the circulation, and, as a matter of course, the part becomes red. Is this inflammation? Yes, in the judgment of many, it is termed a form or variety of inflammation, not strict or positive, but inflammation of an asthenic type, although it fails in three of the four conditions essential to true inflammation, and exhibits redness only, wanting pain, heat, and swelling.

Now, these examples of local congestion or plethora of vessels are yet in the present day confounded with true inflammation, and are treated by what we term antiphlogistic agents—viz., general and local bleeding, purgatives, diaphoretics, and the suspension of food. True inflammation is not so commonly seen as you may imagine; and inasmuch as local redness does not involve all the required signs of heat, redness, pain, and swelling which that state requires, it cannot be true inflammation, and there is no difficulty in proving that the treatment usually adopted in true inflammatory diseases is especially objectionable and injurious in this. There is another variety of so-called inflammation, in the mouths of a large number of us called chronic; what it means is somewhat difficult to define, but we employ it rather to conceal a difficulty than to expound it. There are two principles on which this congested condition of the blood-vessels may be treated.

1. Blood may be taken locally; the system may be lowered by salines, as they are termed, by purgatives and diaphoretics. This treatment by local depletion would lead to the inference that the blood is at fault and should be removed.

2. The blood in these vessels may be forced onwards by giving increased action to the heart, by the resort to agents that tend to restore the healthy tonic condition, or, at all events, to give force to the capillary system of the affected part. This latter is termed the tonic treatment of disease—a principle, I consider, based on a sound view of the functions of the animal economy, and which formed the staple of my treatment during the period in which I was attached to this hospital as surgeon, and for many years prior. If this principle be sound, treatment by depletion in any form must be an error, and therefore injurious, and that it is injurious I have not a doubt. I can speak with some confidence, for I have tried both. In the history of the human mind there is no operation more difficult than that of divesting it of early impressions inculcated by authority and confirmed and established by time. Convictions increase in strength as

we get older, for the habit of examining the one side of a question only, and of conforming to all its requirements, has assumed a settled, permanent form, and with the multitude engaged in the practice of our profession there is no adequate motive to enter on a path of inquiry which may tend to unsettle the convictions of their past lives. Some fifteen years since I had a succession of cases of severe congestion—that is, redness, not inflammation—accompanying wounds caused by operations, cases of mammary cancer or other tumours removed by the knife, amputations, hernia, &c. These cases exhibited on the third or fourth day congestion of the vessels around the wound, which often extended and led to abscesses, and occasionally to death. It was called erysipelas, having the feature of redness only in common with that disease; but it was not erysipelas: in truth, it was a far more formidable condition. I ordered every patient after undergoing any serious operation, treatment by stimulants. I gave two tablespoonsful of brandy and two of water every four hours, to be continued for one, two, or more days. From that date I never lost a patient from this cause, and rarely did any unhealthy condition of the affected region present itself.

Now observe this feature in the treatment by stimulants. If these examples of local congestion of the blood-vessels partook of the nature of inflammation, you, I am sure, will concur with me in considering this treatment a blunder. If stimulants be inadmissible in the treatment of inflammation, the treatment should have proved injurious. This it did not do, but, on the contrary, effected early and curative results; therefore the disease was not that of inflammation. What is true of the local, is equally true of the general.

The late Mr. Jones, of Jersey, was a remarkably successful operator, and in reference to his cases of operation for the excision of joints (considered to be diseased) he operated on a succession of more than twenty-five without the loss of a single case, while in England at the same date the mortality was always great, recovery being rather the exception, and not the rule. What was the secret of this? Was it fresher air, or different management? I made the inquiry, and learnt that Mr. Jones invariably gave every patient on whom he operated at least a pint of port wine on each of the two days following the operation, and he acknowledged to me that he owed the treatment which had been attended with such remarkable results, to his observation of the success that had attended my treatment in St. Bartholomew's Hospital. I have found this combined tonic and stimulating treatment eminently successful, and so convinced am I of its value that, if I were told it had been less successful in the hands of others, I should feel assured it had never been fairly tried. In no disease is the application of this principle more valuable than in every variety of abscess, whether chronic or acute, and especially, perhaps, the latter.

This treatment is essentially tonic in its character. Its object, as I have above stated, is the employment of such medicinal and dietetic agents as tend to restore the constitution to a condition of health and vigour. This is all we can pretend to do, and, rely upon it, nature will do the rest. I do not say exclude all local treatment. Doubtless a few leeches may relieve the pain, and fomentations or poultices may in some slight degree contribute to the same end; but these agents are not, critically speaking, curative—they palliate symptoms only—and as pain is an evil, and itself, when severe, a source of prostration, the surgeon may justly combat it by such remedies as do not draw on the circulation and thus reduce the vital powers of the body.

There are two agents, in addition to good nourishment, especially applicable to the treatment of a case of this kind—bark and wine. There is an inferior class of remedies, but of the same character, such as bitter infusions—cascarilla, gentian, and the like, and the varieties of infusions of malt. There is no comparison between the virtues of the two classes, but as poor persons cannot afford wine, they may afford good beer, of which the strongest is the most efficient; but among medicaments cinchona bark has no rival. In all cases of abscess bark is invaluable. It promotes suppuration when suppuration is inevitable, and it checks suppuration in the exhausting action of chronic abscess. It promotes appetite, and gives vigour to the system beyond any other known tonic, and it is only on this principle that it carries our patients rapidly through the different stages of disease. I prefer the simple tincture of bark, and I recommend you to administer it in full doses of from one to three drachms twice a day. For adults I generally recommend a dessert-spoonful, and not less, and I have not observed that its employment in such doses demands the corrective influence of either purgative or alterative medicine; but on the contrary, as I refer any temporary torpidity of the actions of the alimentary canal to the want of that vigour of system, and to that only, which the bark is capable of correcting, so as a rule, subject of course to occasional exceptions, I leave the bowels alone. I have a very limited faith in the cathartic system. In this I know I am un-English; but I am content to live under the obloquy. On the subject of wine I have no intention of entering on the question how far alcohol is a source of strength or of nutrition. As an ingredient in the diet scale of the invalid it is indispensable; and there is a very important rule of conduct as regards its administration, the knowlege of which I am most anxious to impress on your minds, and which involves a principle that cannot fail to obtain your concurrence on a closer observation of the nature of disease and the remedies pertinent to it. The prominent rule I allude to is this:—Administer wine or alcohol in quantity regulated by its effects on the system, and not in any fixed quantity determined by usage. A gentleman with a weak pulse is told by his physician to take wine freely, and he indulges in a bottle of claret at and after dinner, and by so doing he considers himself to have obeyed the injunction of his medical adviser. Now, I shall select port wine as the standard by which I regulate the advice I give under the above circumstances; and I am of opinion that for the purposes of health three glasses of wine is the maximum quantity that, taken at any one time, can be serviceable. All beyond this answers the purpose of luxury and nothing more, and is more or less injurious. Suppose a person of abstemious habits takes a glass of wine, being much fatigued from exercise, how long will he continue to feel the benefit of it? Possibly, at the outside, two hours or less. The wine did good service at the time; but the effect has passed entirely away. If this person be subject to a renewal of the fatigue, he can take a second glass at the expiration of two hours on the same conditions, and so on. He has in his fatigue the equivalent to disease, and he takes the restorative—with this result, that, having consumed five or six glasses of wine, he is not conscious, judging from his sensations, that he has taken any. If this law be founded in truth, I say administer wine frequently in moderate quantities, or in immoderate quantities if they are required. You can judge for your patient what quantity he can take and what he requires, and I say unhesitatingly, when required, the more he takes the better and the sooner he will be well, which, I presume, is the object to be attained. Judge the quantity of wine then required by its effects, and do not administer it in given quantities. I consider wine indispensable in the tonic treatment of disease. It should be administered in moderate quantities at short intervals. If too short, the effect of the wine will prove cumulative, and reaction will follow. The art of administering alcohol, whether in the form of wine or brandy, is learnt by observation of its effects on each individual; and be assured of this, that the quantity consumed with impunity, be it large or small, is the guage of its want by the system of that person.

During the last forty years the treatment of disease has undergone great and important changes, and up to a later date the use of wine has been very exceptional. I do not overstate the case when I affirm that within the period I have mentioned the consumption of wine and brandy in the London Hospitals has increased fourfold; and I may here relate an anecdote which confirms the above statement. It has, I believe, been before published, but I deem it right to bring it again under your notice. In the year 1848 the treasurer of the hospital commented on the quantity of wine I ordered for my patients. He said the hospital could not warrant the large expense. I inquired the number of patients—550; and the wine consumed was three pipes per annum. I told the worthy treasurer that the consumption of wine quite surprised me; that I could not understand how my colleagues could manage their cases, for that I could not treat hospital cases without wine; and I assured the treasurer that I would do my best, for the credit of the hospital, to raise the consumption of port wine from three pipes a year up to thirteen, and that nothing less would satisfy me or my convictions. Twelve years elapsed, when I was again addressed by the successor of the then treasurer on some matters connected with the diet scale of the hospital. On inquiry into the consumption of port wine, he appealed to the apothecary, who, referring to his wine book, announced the quantity annually consumed to be thirteen pipes! I was on the previous occasion the great delinquent, but now, as the treasurer declared, "You are all nearly equally bad, although *you* still head the list." And the same change in the treatment prevails more or less in every hospital in London. Although this employment of stimulants and tonics at the expense of depletive agents has become so prominent a feature in modern practice, it does not appear to have exercised much influence on the theory of disease as taught in the schools, nor on the practice of the profession among the community.—*Medical Times and Gazette, July* 7, 1866, *p.* 1.

53.—DILUTION OF VACCINE LYMPH WITH GLYCERINE.

By Dr. MULLER.

GEH. MEDICINALRATH MULLER, director of the Berlin Vaccine Institution, has just published a communication concerning vaccine lymph which he regards as of considerable importance. After adverting to the well-known difficulty of obtaining vaccine lymph in sufficient quantity, especially when large numbers have to be speedily vaccinated or re-vaccinated during the prevalence of epidemic small-pox, he observes that it is, therefore, a matter of great importance to be in the possession of a means by which every Practitioner may preserve for himself an ample store of this precious substance. Such a means is to be found in mixing the vaccine virus with diluted glycerine. Without in anywise interfering with its efficacy, this increases its quantity and its power of keeping. Finding that the vaccine scab is best dissolved in glycerine, and impelled to seek for some adjuvatory means by the numerous applications he received from every province of Prussia, as well as from foreign parts, the author was induced to try the effect of some lymph which he had mixed with diluted glycerine, and the results were in the highest degree encouraging. While augmenting the vaccine material ten or twenty fold, the pustules which resulted, neither in their course, appearance, the amount of lymph they contained, or the reaction they gave rise to, differed in any degree from the pustules produced by the purest lymph. The proportion cannot be exactly determined, because the quantity taken up by the pencil cannot be weighed or measured; but as an example it may be stated that lymph from three pustules having been mixed with diluted glycerine, served not only to vaccinate several children, but to charge about forty capillary tubes. Lymph which has been preserved in tubes can be similarly treated with the glycerine, but it will be best only to employ that which has been kept in them for a few days or weeks. How far the dilution of the lymph can be carried without damaging its efficacy is being tried. At present the results have been found uncertain when diluted more than twenty times. Diluted only ten times, they are always certain.

If these statements are admitted, as they must be, it results that the Practitioner may always be in possession of a sufficient supply of reliable lymph, which may easily be stored up, seeing that the lymph enters the tubes with greater facility than in its undiluted state, and keeps much better. With the lymph derived from a single child a whole battalion of recruits may be re-vaccinated. The vaccinator may, too, (not so pressed for lymph as heretofore,) exercise more caution in selecting the subjects for supplying it. On account of its greater preservability, this mixed lymph will also far better bear transmission to distant countries and tropical climates than does the ordinary free lymph. The great ease with which the tubes are filled without contact with the air, and then sealed, will much aid the employment of this diluted lymph. Dr. Müller's exact mode of procedure is as follows:—Having opened some of the pustules of a child vaccinated eight days before, he collects the lymph which issues out upon a small, new hair pencil. The pencil is then moistened upon a glass or porcelain plate by means of from ten to twenty drops of chemically pure glycerine, diluted with equal parts of water—the whole being most thoroughly mixed together by means of the pencil. With this mixture vaccination is immediately performed, or capillary tubes are filled with it and sealed for future use.—*Medical Times and Gazette, May* 19, 1866, *p.* 526.

54.—CASE OF CANCER OF VERY LARGE SIZE OCCUPYING THE SIDE OF THE FACE AND NECK—TREATMENT BY INJECTIONS OF ACETIC ACID.

(Under the care of Dr. Broadbent and Dr. Randall.)

The following case illustrates chiefly the destructive effects of acetic acid injections on malignant tumours of large size and rapid growth; but it is not without instructive suggestions on other points, such as the limitation of the action of the acid to the cancerous structure.

In a case of this kind absorption was not to be looked for, and necrosis was the result expected. The amount of destruction, however, which followed the first injection astonished all who were watching the case. Equally remarkable was the slight amount of suffering attending the process. The absence of fœtor noted is characteristic of this process of treatment. One of the dangers to be anticipated in a tumour of this size in this situation is hemorrhage, but the acid seems to spare the vessels; and as these at first exposed in the neck are now hidden by granulations, there is much less ground for apprehension on this score. As to the future prospects of the patient, the tumour is now limited to the face, in the bones of which it apparently took its rise, and it will be difficult to eradicate the disease at this part, especially as the acid cannot be retained in the morbid structure in any considerable quantity. Further destruction of skin would be undesirable, and to avoid this the later injections have all been made from the wound, which facilitates the escape of the fluid. Up to the present, the treatment is gaining considerably on the disease, and while this is the case, the present cautious mode of proceeding will be persevered in; but, if necessary, more energetic measures can be taken. On the whole, it seems probable that the old woman's life will be prolonged by the treatment, and that it will diminish her aggregate of suffering. The further progress of the case will be reported in some future number.

The patient, an old woman aged 73, had been for some time in the Marylebone Workhouse. The tumour had been growing twelve or eighteen months, and as it had attained a size which might very fairly be characterised as enormous, its rate of increase must have been very rapid. It occupied the right side of the face and neck, the centre appearing to be about over the angle of the jaw. The skin was red and tense, and the tumour was elastic to the feel, but firm. It had occasioned no great suffering for some time at first, but had recently become the seat of some pain, and it interfered with mastication and deglutition. The larynx was pushed aside, and the facial nerve having been destroyed in the progress of the disease, the mouth was drawn to the left, increasing the deformity.

Dr. Randall and Dr. Broadbent decided on the acetic acid treatment, although it was very doubtful whether the patient would have strength to endure the removal of so large a mass, supposing this to be possible. There was reason to hope, however, that the process would be attended with little or no suffering, and it seemed to offer some slight chance of relief in a case otherwise entirely hopeless.

On October 3, therefore, about 40 minims of acid of the strength of one part in four, making 80 minims in all, were injected at two points. The operation gave no pain. Some glutinous colloid matter exuded from the punctures, which matter on examination was found to contain small cells, some of them bi-nucleated, and minute highly refracting particles, scattered and in groups.

For two or three days afterwards there was increase of tension in the tumour, and greater difficulty of deglutition, so that fluids only could be swallowed. A little discharge made its way into the mouth, and the skin then gave way, a large quantity of matter escaping.

Oct. 9. A large opening existed, which was occupied by a loose grey slough; the tumour had diminished very considerably —more than one-half—and the remains occupied only the face, where numerous hard nodules could be felt. The patient could swallow comfortably. She had suffered very little pain, and her strength was apparently not at all diminished. A noteworthy feature in the case was the entire absence of fœtor from the wound or discharge. Two injections of 40 minims each were made into distinct nodules. No pain. A poultice containing acid applied.

16th. Slough entirely separated from lower part of wound, which is now perfectly clean and healthy-looking. The muscles are laid bare, and the carotid is seen pulsating at the bottom of of the cavity. At the upper part of the wound the angle of the jaw is readily made out, covered by a grey soft material. The nodules last injected had disappeared. Patient cheerful and comfortable, swallowing and eating better. No injection till it is seen how the carotid and its branches behave.

19th. Lower part of wound granulating, and opening contracting rapidly. Upper part as before. A little acid was injected into the soft matter covering the angle of the jaw, but this it readily ploughed up and escaped. Injections were practised into remains of the tumour upwards towards the malar bone and downwards over the body of the jaw; the latter only was well retained. The diseased parts to be sponged with the dilute acid.

23rd. The granulations in the lower part of the wound progressing rapidly, and the opening in the skin contracting. A considerable mass of disease still left over the superior and inferior maxillary and malar bones. The sponging with acid seemed to have been of use. A large cavity exists where the successful injection was made on the 16th. Acid injected in various directions from the wound, but little was retained. The patient is cheerful and comfortable, and, on the liberal diet allowed her, gains strength.—*Medical Times and Gazette, Oct.* 27, 1866, *p.* 445.

DISEASES OF THE EYE.

55.—ON REMOVAL OF THE LACRYMAL GLAND AS A RADICAL CURE FOR LACRYMAL DISEASE.

By J. Z. Laurence, Esq., Ophthalmic Surgeon to
St. Bartholomew's Hospital, Chatham, and to the Ophthalmic
Hospital, Southwark.

[It is within quite a recent period that Mr. Bowman's dilating system succeeded the old style, which some eminent surgeons still regard as the best means of cure in cases of obstruction of the nasal duct. In the majority of cases, however, Mr. Bowman's plan proves difficult in its application, and unsatisfactory in its results, and one cannot but be struck with the number of cases which have lasted and been treated for years without the slightest prospect of anything approaching a permanent cure. When in Italy in 1865 Mr. Laurence saw several cases which had been treated by Nannoni's method of destruction of the lacrymal sac, and with very satisfactory results. The operation was also practised twenty years ago by Dr. P. Bernard, but since that time it has never been systematically pursued.]

In the December of last year a man æt. 25, applied to me at the Ophthalmic Hospital, Southwark, on account of an inveterate watering of the left eye from obliteration of the lacrymal canaliculi—the results of a burn by caustic soda some three years previously. No trace of either punctum lacrymale could be seen, and the inner part of the eye-ball was partially adherent to the lacrymal caruncle by old bands of adhesion. I attempted to restore the perviability of the canaliculi by Jüngken's operation; but failing in doing so, I resolved to perform the only other operation that appeared to me to offer any chances of success—namely, removal of the lacrymal gland. I found this operation perfectly easy. Within four days after the operation, the watering of the eye had ceased; nor was the eye unduly dry.

On September 25th, 1866, about nine months after the operation, I saw the patient again, and was able to verify the permanence of the relief up to that period.

The facility and success of my operation stimulated me to inquire how far it was applicable to cases of inveterate lacrymation generally. This inquiry is at present on but a limited scale; its results are, however, so satisfactory, that I have ventured to submit them to the profession.

Emma N., a child about three years old, was brought to me at the Ophthalmic Hospital, Southwark, for watering of both eyes, and lacrymal abscesses of about a year's standing. I, in the first instance, tried to dilate the nasal ducts with probes, after Mr. Bowman's method; but found, partially no doubt on on account of the child's intractability, the method quite inapplicable.

I then attempted to obliterate the left sac by Nannoni's method, with the solid perchloride of iron, but met with no better success.

On March 6th, 1866, the left eye was continually watering, and pressure over the lacrymal sac caused a slight amount of pus to exude from a lacrymal fistula, and a very large amount from the puncta lacrymalia. I removed the lacrymal gland, and within a week from the operation all watering of the eye had ceased.

On March 16th, I removed the other lacrymal gland for a nearly corresponding condition of the right eye; within a week the watering of the right eye had ceased. I saw the child many times after the operation. On the 25th of September, not a trace of lacrymation remained; the lacrymal fistulæ are quite healed. A little pus can be pressed out of the puncta of either side by pressure over the lacrymal sacs. Both eyeballs had ever since the operations continued perfectly moist and normal in all respects. A curious, but natural, sequence of this operation was observed in this case very markedly, namely, the complete inability to shed tears. This child always set to crying most lustily whenever the mother brought her to me, but never shed a single tear, however much she shrieked and cried. For a little while immediately after the operation, this patient suffered from a slight degree of ptosis, but this gradually entirely disappeared.

[A summary of the main features of six other cases is then given; after which the author continues:]

I have removed nine lacrymal glands in eight cases. In these cases the following results of the operation deserve some notice. The disappearance of the watering of the eye was immediate and permanent, as far as my observations extend. A remarkable feature in this point of view was, that this ensued whether the gland was completely or only partially removed. Indeed, in one case, which I have not recorded, that of an infant, I could not succeed in removing the gland ; but curiously enough, although the eye had previously watered since birth, within eleven days after my aborted operation, all lacrymal symptoms had ceased, and when the child was last seen, forty-nine days after the operation, the cure had remained permanent. These facts suggest the idea whether a simple division of the ducts of the gland might not suffice for the cure of these cases? Some such idea occurred also to Szokalski, when he proposed to ligature the ducts of the lacrymal gland, and thus induce its atrophy.

The first question every one who has heard of my operations has asked me, has been, does the eye continue moist after the operation? It does, completely so. My experience in this point of view, is corroborated by that of Mackenzie and of others, who have removed the lacrymal gland on account of tumours of its substance. Thus, Mackenzie states, "the moisture and lubricity of the conjunctiva remain unaffected after extirpation of the lacrymal gland." In Dr. O'Beirne's case, in which the lacrymal gland was removed on account of its enlargement and induration, the patient suffered no inconvenience. Nor did Mr. Dixon's patient, who underwent the operation on account of traumatic obliteration of the canaliculi.

Another remarkable consequence of removal of the lacrymal gland for abscess of the sac is, that the abscess and discharge of pus through the puncta gradually cease. This I regard as analogous to the cessation of cystitis following dilatation of stricture of the urethra. The sac is no longer irritated by over distention by the tears, which, also, doubtless, acquire acrid properties from their undue retention, and thus the lacrymal cystitis gradually ceases.

In most of my operations, which have been performed by the external operation, a slight degree of ptosis has followed the operation, but gradually disappeared. This ptosis is due to two causes:—firstly, a partial division of the levator palpebræ : and, secondly, œdema of the upper eyelid.

I consider removal of the lacrymal gland applicable to those cases of inveterate lacrymal fistula, which other methods, after a fair trial, have failed to cure, the operation offering the best prospect of a radical and permanent cure.

Some of the cases I have narrated might, doubtless, have been cured by other means. But my object has been to reopen a field of experimental inquiry to solve the question—Will removal of the lacrymal gland cure lacrymal abscess? Bernard's and my own cases prove that it will. I intend prosecuting this inquiry in all fit cases that occur to me. For what class of cases the operation is specially applicable, is another question, which subsequent research must determine.

The method that I have followed in performing the operation has been to make a transverse incision of about ¾ in. into the orbit, below the upper and outer third of the orbital ridge. After a little careful dissection the gland may be found in the wound by the little finger, laid hold of by a small sharp double hook, brought forwards into view and removed. With one exception, the wound in my cases healed by the first intention. The linear scar of the incision is inappreciable, being lost in the folds of the upper eyelid.

From some operations I performed on the dead subject, I thought that the gland might be more easily removed by an incision from the inside through the conjunctiva of the upper palpebral sinus. On attempting to put this plan into practice, I found that the readiest mode of exposing the gland was by a combination of the internal and external incisions. After making the external incision, I divide the external commissure of the lids with scissors, and by connecting the outer ends of the two incisions, form a triangular flap, which I throw up. In this way the lacrymal gland is easily exposed. I have never had any troublesome hemorrhage. An artery of the orbit (probably the lacrymal branch of the ophthalmic) bleeds the most freely; but this has, in my cases, soon stopped on the application of cold water. The difficulty of finding the gland varied in the cases I have operated on according to the depth it lay at, and the amount of fat in the orbit.—*Ophthalmic Review, Oct.* 1866, *p.* 138.

56.—ON RHEUMATIC IRITIS.

By James Rouse, Esq., Lecturer on Anatomy at St. George's Hospital School.

[By rheumatic iritis is meant that form of inflammation produced by sudden changes in the atmosphere, whether thermometric, from hot to cold, or hydrometric, from dry to moist. In these cases there may be presumed to be an acid state of blood, manifesting itself by inflammation and effusion into the joints, by acid secretions from the skin and kidneys.]

The diagnosis of rheumatic iritis is not a difficult one: the history of the case and the appearance of the patient are the main indications. Much has been said by authors about the danger of confounding iritis with rheumatic or catarrho-rheumatic ophthalmia, with corneitis, with retinitis, &c.; but it appears to me that very slight knowledge of the subject will enable a surgeon to steer clear of this danger. I must, however, admit that the diagnosis from certain cases of syphilitic and strumous iritis presents some difficulty; as in the former, patients will often resolutely deny the occurrence of primary disease, unless an eruption remains, which will settle the question. It may be asked, Can you distinguish between rheumatic and syphilitic iritis by the appearance of the eye alone? Only in certain cases, I believe. where the iritis is chronic and the ophthalmoscope can by used, this instrument displaying syphilitic deposit on the retina. German authors state and believe that in syphilitic iritis the pupil is always displaced upwards and inwards. In order to test the value of this statement I carefully recorded a number of cases of syphilitic iritis; and I find that the iris is less frequently displaced upwards and inwards than in any other direction; clearly, then, we cannot depend on the displaced pupil as a means of distinguishing between these two varieties of the disease. There is in the syphilitic affection less conjunctival inflammation, less intolerance of light, and a greater tendency to deposit of larger masses of lymph on the anterior surface of the iris, also a greater tendency on the part of these masses when formed to degenerate into pus. In strumous iritis the diagnosis is easier, as this disease is always the sequel of other strumous affections of the eye.

The prognosis in slight cases is most favourable, as the eye usually entirely recovers the effects of the inflammation; but when the patient has been neglected, and the iris has become completely adherent to the capsule of the lens, a very cautious opinion should be given; for although in some instances all the effused lymph has been absorbed under the use of mercury, the more common result is lasting damage to the eye.

Of all the sequelæ of iritis none are more common than permanent adhesion of the pupil to the capsule of the lens. This adhesion may be (1) total, the posterior chamber no longer communicating with the anterior; or (2) partial, little delicate bands passing from the pupillary margin to the capsule of the lens. Besides the pupil being changed in form and rendered immovable, lymph may be deposited on the capsule, giving rise to what is called cataracta lymphatica, or spurious cataract. Now these adhesions, whether total or partial, give rise to one of the most troublesome complications the oculist is ever called upon to treat—to wit, recurrent iritis. Fortunately we are now able to prevent, by an operation, the destruction of vision which would sooner or later occur from this complication. There is another and equally serious affection which not unfrequently is a sequela of rheumatic iritis, viz., irido-choroiditis. The symptoms indicative of this disease are, in addition to those of iritis, increase of nocturnal pain, sparks or rings of light continually passing across the sight, gradual and constantly increasing intra-ocular pressure, then glaucomatous hardness of the eyeball, and if this be not relieved, destruction of vision.

Before entering into the question of what remedies we are to make use of, we must remember that the chief objects we aim at are : 1, to subdue inflammation ; 2, to prevent effusion, or if effusion has already taken place, to prevent its organisation ; 3, to preserve the pupil, and relieve the severe pain which usually exists. In the treatment of iritis, there is no remedy, if given judiciously, that produces so great an effect in subduing inflammation, and in preventing the effusion of lymph, as mercury. I find two grains of calomel, and a quarter to half a grain of opium, the best and most convenient form of administration ; but it must be always borne in mind that salivation is not the effect required, and except in the slightest degree, it is rather to be avoided than promoted. The favourable result of this treatment mainly depends upon its being commenced early in the disease.

Unfortunately patients seldom apply for advice early ; consequently we are called on to treat cases where lymph is not only effused, but is becoming slowly consolidated, and yields but slowly to treatment. In these cases mercury has to be given for a long period,* and the gums kept just spongy. If this be done, the lymph, in a great measure, becomes absorbed, at worst a few tags only become organised, and a good recovery is made. There are, however, many cases in which large doses of mercury should be avoided, and the drug administered only in very small quantities—to wit, in persons of broken-down health or of phthisical tendencies. Under these circumstances I find a combination of two grains of quinine, three of blue-pill, and three of extract of hyoscyamus, most beneficial. I have had recently under my care a young strumous patient afflicted with rheumatic iritis, in whom a perfect recovery, without apparent damage to his general health, has taken place under such treatment. A very important question arises in relation to the period when mercury may be discontinued with safety. I hold a very strong opinion myself, that it should be persevered with so long as the sclerotic zone of vessels is visible ; for so long as this symptom continues, a relapse may take place.

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* In these cases I give the following prescription : ℞. Pot. iodidi, gr. x. ; liq. hydrarg. bichlor., ʒj.; decoct. cinchonæ ℥j.: ter quotidie.

We must now pass to the consideration of the use of belladonna in these cases. Most authors recommend its use in every case, and in all stages of the disease. Gräfe says: "the severity of the pain is by no means a contra-indication for this treatment: for the most severe ciliary neurosis often diminishes when the pupil dilates." "It should be applied daily, six, eight, or ten times, or oftener, at intervals of five minutes; and notwithstanding the irritation, both the pain and the other symptoms are generally alleviated." In very acute cases Gräfe applies atropine twenty or thirty times a day. Contrast this with the opinion of Mr. Dixon, one of the soundest practitioners in this country. "I do not regard it (atropine) as of any service in iritis; for, as I stated, an inflamed iris loses its power of motion. Atropine, therefore, must be useless during the active stage of inflammation. At a later period, when the iris is beginning to recover its motory function, it may, I think, even do harm; and in the following way: the hinder surface of the iris, termed uvea, is covered with a layer of pigment-cells; when fibrine is poured out behind the iris, these pigment-cells become for a time firmly united to the capsule of the lens; and if, when the iris is regaining its motory function, a forced dilatation of the pupil be effected by the influence of atropine, some of the pigment may be detached from the posterior surface of the iris and left adhering to the capsule, forming those brown patches so familiar to us in patients who have suffered from iritis."

I have quoted at some length these opinions, as showing how two men, both of vast experience, may differ on a point of treatment apparently so easy of decision. During the last seven or eight years I have had a very large number of these cases under my care; and I have adopted the following plan, which appears to me to answer very well. In the early stage of the disease—that is, before the lymph is consolidated—a solution of sulphate of atropine, four grains to the ounce, is used every night and morning. When the attack becomes very acute, it is discontinued, because I have found (as Mr. Dixon states) the adhesions ruptured, and the pigment left behind on the capsule of the lens. When the lymph is being absorbed I again use the solution, as it dilates the pupil and stretches the bands attached to the margin of the iris. These at last give way; and as they are nourished by the iris (as shown by Gräfe), become atrophied when detached from it.

From these remarks on belladonna it will be perceived that I do not believe in its curative effect, but that I consider it only as a useful auxiliary to other remedies.—*St. George's Hospital Reports, Vol. I*, 1866, *p.* 105.

SYPHILITIC AFFECTIONS.

57.—A NEW REMEDY IN GONORRHŒA.

By Dr. J. S. Prettyman, Milford, Del., U. S.

[The oil of erigeron was first used by Dr. Prettyman in hæmoptysis, and it was from this that its use in gonorrhœa suggested itself to him. He finds it to act "with unvarying success." It is a volatile oil obtained from the *Erigeron Canadense*, and has also been found of use in diarrhœa and various hemorrhages. There are three species of erigeron, or fleabane, officinal in the United States Pharmacopœia, and they are used in the forms of infusion, powder, and watery extract. Speaking of the oil of erigeron the author observes :]

It arrests the discharge in about 72 hours, and effects a cure in from six to eight days. I do not recommend it as a specific in all cases, but design merely to bring it to the notice of the profession as an exceedingly valuable medicine in this disease. Of course all scientific medical practice is based upon the well-known pathological condition of the structures involved, and this is our unerring guide. When, in recent cases, the urethral inflammation is severe, my plan is to precede the remedy with a full dose of some active hydragogue. A good formula is:—℞. Pulv. senna, ℈ij; pulv. jalapa, ℈j; pulv. aromaticus, gr. x. M. Add a gill of boiling water and a teaspoonful of sugar, and, when sufficiently cool, agitate, and swallow at a dose. As soon as this operates, give ten drops of the oil on sugar, and three hours later a full dose of spts. æther. nit. in infus. althea, and so on every three hours alternately until the urethral irritation is allayed. Then leave off the latter, and continue the oil until the cure is complete. If the case is not recent, or there is but little urethral irritation, the oil alone is sufficient.

I have used it also in combination with copaiba and other articles, and found such preparations to answer a good purpose, but no better than the oil alone.

The oil which I use is reputed to be that of the *Erigeron Canadense;* but I presume that from the *Philadelphicum* is equal, if not superior, for this purpose.—*American Medical Journal, July*, 1866, *p*. 117.

58.—ON THE OIL OF YELLOW SANDAL WOOD IN THE TREATMENT OF GONORRHŒA.

By Dr. H. Samuel Purdon, Physician to the Belfast Dispensary for Diseases of the Skin and Chest, &c.

Having for some time used the oil of yellow scandal wood in the treatment of gonorrhœa, I take the liberty of laying the following observations before the profession.

We are indebted to Dr. Thomas Henderson, of Glasgow, for introducing to our notice during the summer of last year this oil, at present used extensively used as a perfume. This oil is obtained by distillation from the wood of the tree *Sirium myrtifolium*, a native of the East Indies, "one pound of the wood, yielding about two drachms of oil." The dose being ten to thirty drops diluted with rectified spirit, but as it is liable to sicken when thus administered, I now combine it with various other remedies as hereafter mentioned.

I have now tried this remedy rather extensively in private practice, as its price renders it too expensive for either hospital or dispensary use. The following facts differ slightly from Dr. Henderson's conclusions:

"Dr. Henderson states that the oil of yellow sandal wood acts as a stomachic medicine, occasioning little nausea."	In many cases I have had to discontinue its use, on account of the nausea it occasioned.
"Has slightly any smell."	The odour of this oil is extremely powerful, remaining on the breath, hands, &c., even after being frequently washed. It is also evident in the urine, and in one case I remarked a sickening odour emanating from the penis, and which was very annoying to the patient.

The above are the only objections I know of, and when we take into consideration the rapidity with which it cures, as contrasted with either cubebs or copaiba, it may be looked on in the light of a specific, but I think it right to mention that I know of two cases when its use certainly aggravated the disease. This oil of yellow sandal wood may be used in any stage of gonorrhœa, and the following are the formulæ that I usually employ.

℞. Olei santal. flav., ʒiij.; ess. cinnamomi, ʒj.; spt. rect. ad ℥ij. M.

Sig. "A teaspoonful three times a day in a wineglassful of water, after food."

In some cases the compound spirit of lavender occasions the oil to agree better with the patient Bearing in mind the maxim that "union is strength," it occurred to me that by adding oil of savin, copaiba, &c., a mixture might obtained, which would without doubt be a "specific" and the following I have often used with the best results.

℞. Olei santal. flav., ℥j.; olei sabinæ, ʒj.; olei cubebæ, ℥ss.; olei copiabæ, ℥j.; ess. cinnamomi, ʒj.; spt. rect. ad ℥vj. M.

Dose the same as the preceding.

This is a very powerful mixture and usually cures in a few days. In cases where copaiba has failed the oil of yellow sandal wood frequently acts "like a charm," and I very seldom use injections in any case of gonorrhœa. This oil is also useful in gleet and we may often hasten the cure by administering at the same time ergot of rye. I have tried this oil in chronic bronchitis accompanied by profuse expectoration, but without any good results.

Case 1.—J. H. C., aged 26, consulted me in September, 1865, for a gleet of three years' duration, and for which he had been taking various remedies. Considering this a good opportunity for trying the oil of sandal wood, I ordered him the mixture No. 1. Coffee and spirits were forbidden. The dose of oil was gradually increased, its use being only omitted once, owing to a slight dyspeptic attack. At the end of eight weeks this patient was completely cured.

Case 2.—J. M., consulted me in December, 1865, for a gonorrhœa which had existed five days. I ordered him the mixture No. 2, under the use of which every symptom disappeared at the end of four days.

Case 3.—J. E., æt. twenty-one, of a cold lymphatic temperament, consulted me November, 1865, for a gonorrhœa of about one week's duration, being the third he has had; he has always experienced considerable difficulty in getting cured. I ordered him mixture No. 1, with an injection of a strong effusion of green tea twice a day. At the end of the week being no better, mixture No. 2 was ordered, but as the patient did not like it, returned to No. 1, under the use of which the disease disappeared.

Case 4.—H. J., aged 24, of a strong and healthy appearance, but as I subsequently learned of very dissolute habits, consulted me March, 1866, for a virulent gonorrhœa, for which he had been taking copaiba capsules. I put him at once on mixture No. 2 and had him cured in a fortnight.

Case 5.—E. W., aged 18, consulted me also in March, 1866, for a first gonorrhœa, which only appeared two days previously, As he was rather frightened he promised to abstain from spirits. &c. Ordered mixture No. 1, under which he was cured in seven days.

The above cases have been selected from a few others in which the same treatment was pursued. I have no theory to offer on the mode by which this oil cures, but leave the facts to speak for themselves.—*Medical Mirror*, *Sept.* 1866, *p.* 543.

59.—COPAIBA DEPRIVED OF ITS DISAGREEABLE SMELL.

Copaiba and pitch, of each one ounce; magnesia, a sufficient quantity to make a mass. According to the greater or less amount of magnesia, the mass will be more or less consistent. If the latter, a teaspoonful may be given two or three times a day; if the former, pills may be made. M. Beyran, of Paris, has found this preparation not only effectual, but to present none of the usual and very offensive odour of the copaiba.—*Lancet, July* 8, 1865, *p.* 34.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

59.—A NEW METHOD OF INDUCING PREMATURE LABOUR.

By Dr. Lumley Earle, Obstetric Surgeon to the Queen's Hospital, Birmingham, &c.

The plan which I am going to submit to the notice of the profession is not intended to supersede all the well-known modes of inducing labour, and probably a method never will be devised which will be equally suitable, although it may be applicable, in all cases.

Medical men often have what they call "a favourite" method of performing the operation, and adopt it in all instances; this I believe to be a mistake. The plan selected should depend upon the nature of the case—e.g., rupture of the membranes is preferable in severe cases of general anasarca, which disease is generally associated with dropsy of the amnion, and in accidental hemorrhage. Ergot is most suitable in nervous women who obstinately refuse the use of "instruments," or any local means. Kiwisch's method of injecting water forcibly against the os uteri ought to be exclusively used in those rare cases in which such extensive deformity exists, or so large a tumour fills up the cavity of the pelvis, that the os uteri cannot be reached by the finger; and in retroversion of the uterus, if a catheter or other instrument cannot be insinuated within that organ. Dr. Barnes' india-rubber bags are most useful in all instances where it is necessary to commence and terminate the labour in a few hours, as in placenta prævia, puerperal convulsions, &c. Separation of the membranes from the walls of the uterus by passing an elastic catheter between the uterus and the membranes, and either leaving it there, or removing it after having slightly moved it from side to side, is best adapted in cases in which there is only a moderate contraction of the brim of the pelvis.

The new plan is intended to remove certain objections attached to the two modes of inducing labour with an elastic catheter. The objections are these:—If the catheter is simply passed between the membranes and the uterine wall, moved from side to side, and then removed, it does not always induce uterine action, and it requires more manipulative skill and experience than most practitioners possess. In the hands of Dr. Arthur Farre, it has proved most successful; but in the hands of others it has often failed. If the catheter is passed in a similar manner, and left in the uterus, and tied to the patient, although it is much more certain in its action, the patient has to stay in bed until labour comes on, as it would not be altogether safe for her to walk about with the instrument retained. It is very advisable that the patient should be able to go about her ordinary duties until uterine action sets in, because her thoughts then are not so liable to be fixed exclusively upon the recent operation or the forthcoming delivery.

The principle upon which the new method acts is exactly the same as that of the catheter—viz., by separation of the membranes, and irritation. The instruments consist of—

1. One or more pieces of native india-rubber tubing of the length and size depicted in the engravings. One end is closed and the other open. To the open extremity is attached a piece of strong thread to enable the tube to be removed when necessary. Fig. 2 shows a longitudinal section of the tube.

2. A whalebone sound or guide fourteen inches long. Fig. 3 represents the tapering extremity of the guide, which must be made to fit the tubes so loosely, that if the two were inverted the tube would fall off immediately. At the same time, the point of the guide must not be made so fine as to endanger the structure of the india-rubber.*

Labour should be induced as follows :—Place one of the tubes on the tapering end of the whalebone guide (Fig. 1), which must be held lightly by the right hand. Then guided by one or two fingers of the left hand, pass the tube into the os, and gently push it on between the membranes and uterine wall for several inches, as far as it will go readily. No more force should be used then is required in passing a catheter into the female bladder. If any resistance be met with, it should be overcome by passing the tube in another direction. Lastly, slowly remove the whalebone guide, leaving the tube in the uterus. One or more tubes may be passed in the same way, but in a different tract. The patient need not be undressed, and, after remaining quiet for about an hour, may get up. The hydrostatic pressure of the liquor amnii below the india-rubber tube or tubes will prevent them from readily falling out of the uterus. If in twelve hours the tubes are still in the uterus, and labour pains have been felt, they may be removed by pulling the string.

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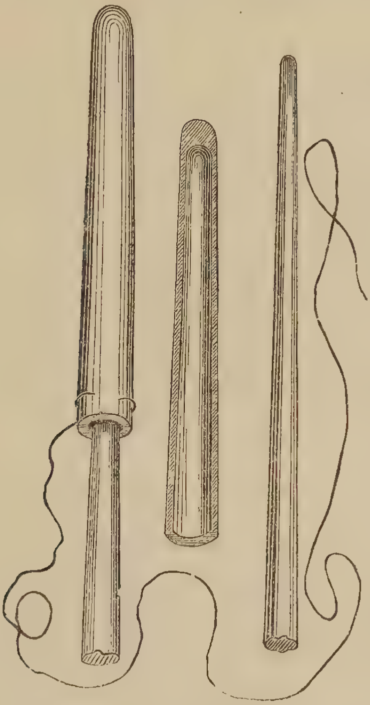
* The whalebone guide, with six india-rubber tubes in a neat paper box, can be obtained from Mr. T. P. Salt, Bull-street, Birmingham, price 5s.

The string should be just long enough to hang about three inches out of the vulva, and the patient should be warned not to let it get entangled in her clothes—in fact, it would be better to pass it into the vagina.

FIG 1. FIG 2. FIG 3.



The advantages of this method are:

1. Its safety both to mother and child. The india-rubber tubing is soft and compressible, and during its passage into the uterus is much less likely than the catheter to rupture the membranes on the one hand, or to injure the uterus on the other.

2. The patient can walk about, retaining this small foreign body in the uterus, without any risk.

3. The operation is very easy to perform, and does not require more than ordinary skill.

4. It is not only safer than the catheter left in the uterus, but probably more efficient than the catheter when it is merely passed into the uterus, and withdrawn after having been slightly moved from side to side, as it acts both by separating the membranes and also by irritation, not set up through any offensive emanation due to decomposing fluids, but by the mere presence of a foreign body.

5. More than one tube can be passed into the uterus with safety, thereby separating the membranes more extensively, and setting up irritation in more than one place.

6. The whalebone guide is preferable to a metallic sound, because, on account of its flexibility, it facilitates the introduction of the india-rubber tube into the uterus. If any obstruction is met with, the whalebone immediately bends, and the smooth and yielding nature of the india-rubber causes it to take a more favourable direction.

I can only bring forward at present one case in proof of its efficiency; and although it is unadvisable in most instances to urge the value of a new method of treatment from its success in one case, I consider this new plan to be an exception to the rule, as it is founded upon an old principle which has been well tried and approved of. From what I know of the action of the catheter in the uterus, I felt as certain as I could be without trying it that it would succeed, and I should have been surprised if it had not.

Mrs. L., aged 37, a married woman, residing in Newhall street. The following is her obstetric history :—The first two deliveries were good. The third, fourth, and fifth were very bad, each increasing in difficulty, but not instrumental. The sixth and seventh labours had to be terminated by craniotomy, extraction with the forceps first having been attempted. In the eighth pregnancy premature labour was induced by Mr. Clay, of this town, and delivery effected by turning. In the ninth and tenth pregnancies I brought on labour both times by passing an elastic catheter between the membranes and the uterus.

The present was her eleventh pregnancy. According to her calculations she had gone just seven months. I first intended to have waited at least a fortnight before performing the operation, as her children at the seventh month were unusually puny and weak, and either were stillborn or died within twenty-four hours. On examination, however, I found her to be very large, as if she had very nearly gone her full time. Only one fœtal heart could be heard beating, but there appeared to be two placental bruits, one at each side of the fundus. The condition of the os was such as to confirm the patient's opinion that she was in the commencement of the seventh month of gestation. My idea was that she was either carrying twins, or that there was dropsy of the amnion. Not feeling certain, and fearing lest she should have miscalculated, I thought it would be better to induce labour in a few days. Accordingly, on Saturday, June 9, at 9.30 a.m., in the presence of Mr. George Jones, who kindly gave me the benefit of his advice, I passed up two india-rubber tubes between the membranes and the uterus. This was very easily done, and the patient did not complain of pain. At 12 a.m., while walking about, one of the tubes came away. In passing the second tube I did not take particular care to pass it into a different direction from that of the first, and it probably passed along the same track, and, not being far from the os uteri, more readily came away. At 7 p.m. the pains commenced, and were repeated at intervals of about half an hour. At 10½ p.m. I removed the remaining tube. Like the first, it was full of blood mixed with a sero-gelatinous material, and there was also a slight discharge of the same fluid. No particular change was noticeable in the condition of the os. On visiting her next morning at 11.45, I was informed that the pains now came on every quarter of an hour, were strong, and lasted for some considerable time. The os was somewhat dilated, but not sufficiently to enable the presentation to be made out. There was a slight brownish discharge, which appeared like blood which had undergone a certain amount of extravasal change, mixed up with a gelatinous secretion. It was not offensive in the least. At 6 p.m. I was called to see her. The pains were recurring every five minutes, and the os was dilated to the size of a florin. The finger could not reach the presentation in the ordinary way, and therefore I did what I always do when uncertain as to the position of the child—I passed up the hand into the vagina and felt the head presenting. At 9 p.m. the os was fully dilated. The membranes were very tough, and I was obliged to rupture them with Lee's trocar. The child was born naturally at 9.40; it was alive, but very small and feeble. On examination I felt the head of a second child presenting. The pains soon recommenced, and each was attended with severe hemorrhage. I then ruptured the membranes, which were also so tough as to require the use of Lee's trocar. As the hemorrhage continued notwithstanding, and the head was still high up, not apparently making any progress, I thought it advisable to deliver by turning. The second was stillborn, and the first died two hours after; both were males. It was unfortunate that the case turned out to be one of twins, as of course the children were smaller and more feeble than if it had been a single birth. The mother made an uninterrupted recovery.—*Medical Times and Gazette, July* 14, 1866, *p.* 32.

61.—ON COMPOUND ANÆSTHETICS IN MIDWIFERY.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea, Brompton, and Belgrave Dispensary.

[It is in actual practice an extremely difficult thing to avoid an overdose of chloroform in midwifery cases by any method hitherto devised; and, moreover, the pains are interfered with, and the parturient effort really protracted. By mixed vapours, as recommended by Mr. Ellis, these dangers are completely avoided, and the expulsive efforts are really assisted by the gentle stimulus these vapours communicate.]

My present purpose is to introduce a new and most simple form of my mixed-anæsthetic inhaler, which will be found, I trust, of value in the lying-in room. This I call the "Obstetric Inhaler." It combines all the excellencies of my former instrument, but is much cheaper, less complicated, and has the great advantage of being quite self-acting and self-supplying. It may be used either for pure chloroform, or for the mixed vapours, as may be required.

It consists (the face-piece, &c., being all as usual) of two parts —an upper, which holds the chloroform or other anæsthetics, and a lower for the alcohol and general evaporation of the fluids. The upper part is an entirely new little apparatus which I have designed and perfected for dropping chloroform in an equal ratio per minute: this I call the "chloroform-dropper." It consists of a glass bottle of a peculiar construction, perforated at both ends. Up its centre runs a double metallic tube, the inner one of which contains a few strands of wool, and the outer serves as a cap to prevent spilling and irregular action of the apparatus. This is held in its place by a cork fitted into the lower end of the bottle. The upper end has a flat stopper, removable to allow of pouring in fresh supplies of fluids. Now, when a definite quantity of chloroform is poured into this bottle. the wick immediately causes it to rise up the tube, and, descending within it, the fluid appears below in regular drops. The rate at which the fluid comes over is easily learnt by calculation, and there is an engraved scale on the side of the bottle which accurately informs the operator how much fluid per minute is escaping from the bottle, and the consequent per-centage of chloroform vapour which his patient is inhaling. Nothing more is necessary in order to give chloroform at a rate of from one to three per-cent. than simply to pour the fluid, at intervals of five or ten minutes, into this little reservoir, up to the indicated mark for the per-centage requisite. The beautiful regularity with which this most simple instrument performs its required office must be seen to be fully appreciated. At its highest power it can only give off three per cent. of chloroform. Absolute security is thus obtained, while—a much lower per-centage being, in my opinion, quite sufficient—this can be perfectly secured by keeping the fluid at a certain point by occasional replenishment. Its tendencies, also, are all in the direction of safety, for its rate diminishes, with great equability, from three per cent., until, if left unsupplied, it finishes with about a half per cent. of the chloroform. Thus the patient, once anæsthetised, may be kept in that state with the greatest safety, since at every few minutes she derives a smaller and a smaller quantity of fluid from the apparatus. The lower part consists of a simple metal cylinder, the interior of which is occupied by my arrangement of leaf-like processes for the evaporation of alcohol. Upon this fimbriated structure the chloroform drops, and passes with the alcohol and ether vapour into the respirations of the breather. To this part of the apparatus is attached the elastic tube and face-piece. These have been well made by Messrs. Savigny, the makers of my other apparatus.

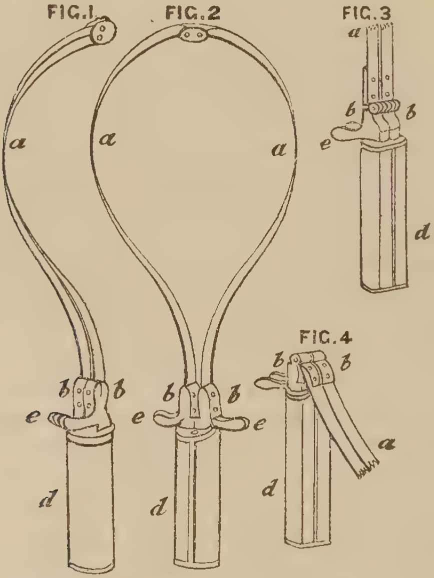
In actual practice the instrument is thus used. A certain portion of pure alcohol is poured upon the evaporating surface in the cylinder; this lasts for about ten minutes. The chloroform-dropper is supplied up to the required mark on the scale with the fluid, and immediately delivers its contents drop by drop over the evaporator, on to which a few minims of ether are occasionally poured; or ether and chloroform in equal parts may be poured into the dropper, and coming over in single drops are dissipated into vapour with wonderful regularity. But the alcohol cannot be thus mixed; it is always to be poured on to its own proper surface.

The instrument is thus entirely self-acting, and to a great degree self-supplying. It is incapable of derangement by agitation or otherwise; it affords a perfect security to the patient, who cannot draw from it more than a known (and visible) percentage of chloroform; and it is almost as simple as the commonest kind of inhaler. So soon as these facts shall become known, and mixed anæsthetics more generally used in our profession, I believe the napkin and tumbler system of giving chloroform, especially in midwifery, will drop out of knowledge and practice.—*Lancet, June* 30, 1866, *p.* 708.

62.—THE STEEL FILLET OF G. R. SHERATON, ESQ.

Sedgefield, Durham.

This instrument is intended to supersede the forceps, vectis, &c., in cases of difficult parturition where the use of these instruments is indicated. It is constructed by combining a rotatory action with the fillet principle. It consists of two blades (*a*, Figs. 1, 2, 3, 4) of highly-tempered and flexible steel, curved in a somewhat sigmoid form, fitted and flexed into the rotatory bars (*bb*) of the handle (*d*) at one extremity, whilst at the other they are united by being riveted into a linked joint (*c*), which allows each blade to rotate in opposite directions to the extent of 90°. The rotatory bars are also formed and fitted with stops to limit their motion to 90°. The rotatory bars rotate upon the handle (*d*), to which they are fixed by nuts being screwed upon their ends to retain them in their position. The handle (*d*) is flattened at the back part to enable the operator to determine with accuracy its relative position to the fœtal head and maternal passages. The rotatory action is obtained by pressing upon the transverse bars or "lugs" (*e*), by which the blades are formed into a loop (Fig. 2) of elliptical form, having a short or transverse diameter of 4¼ inches and a long diameter of 5½ or 6 inches. Tractile power is obtained by placing the fingers across the transverse bars. Its application may be effected by the blades being parallel (Fig. 1) or a little apart,



forming a sort of fenestrum, and is to be introduced in the same manner as one blade of the forceps or the vectis, using the same precautions and preliminary measures as with those instruments. It may be applied over the occiput or chin, as desired; then the blades are to be made to rotate by pressing upon the transverse bars, when it is converted into a loop embracing the head. Its application is facilitated by the form of the linked joint, which renders the instrument, as it were, probe-pointed, whilst its being moveable renders it less liable to catch folds of the maternal passages, &c. The flexibility of the blades allows it to adapt itself to the axis of the pelvis and to the form of the head. The thinness of the blades, from the small amount of space which they occupy, is also calculated to facilitate its application. The fingers are to be placed across the transverse bars, and tractile force applied in the direction of the axis of the pelvis. The amount of compression upon the head is always in proportion to the amount of force required in extraction, and from the manner in which that compression is applied it is not liable to cause injury to the child.

The chief advantages which this instrument seems to possess over the forceps are—

1. Its application is easier. *a.* The narrowness of its blades. *b.* The thinness of its blades. *c.* Its flexibility.

2. That one blade is easier of application than two.

3. It is less liable to injure the maternal passages or the fœtal head.

4. It occupies less space than the forceps.

5. It can be used without the knowledge of the mother.

6. From the facility with which it can be applied, the duration of the operation is shortened.

It may be made with a folding joint to render the instrument more portable, as Figs. 3 and 4. — *Medical Times and Gazette, July* 21, 1866, *p.* 74.

63.—ON THE MANAGEMENT OF "RETAINED PLACENTÆ" IN ABORTIONS.

By Dr. C. B. Suckling, Senior Surgeon-Accoucheur Queen's Hospital, Birmingham.

M. J., aged 29, married; fifth child; six months advanced in pregnancy; has hitherto carried her fœtus to the full time of utero-gestation. She was taken with uterine pains early on Saturday morning, February 3, 1866, which continued on and off until I was summoned to see her on the following Monday at 10 a.m. I saw the patient at 10.30, and found she had had pain more or less intense during the previous three days. She had lost but very little blood. A vaginal examination, which is the only one to guide us to an accurate diagnosis in these cases, was at once made. It revealed the presence of the fœtus in the vagina, and a patent state of the external opening of the os uteri. On removing the fœtus, the umbilical cord became detached from the placenta, three-fourths of it being left adherent to the fœtus, and the remaining fourth to the afterbirth. This severance was afterwards found to be owing to a state of decomposition which had set in apparently at the time when the pains had been first felt by the patient. As the afterbirth could not be felt, and as there was no flooding, after waiting a little time, I adopted my usual course—plugged, and left the patient, promising to return in the course of seven or eight hours; but I requested the attendant to send for me immediately, should anything occur of an alarming nature during my absence. When I visited the patient again at the expiration of eight hours, I found she had been pretty comfortable, free from pain, and had been soothed by sleep. She told me she had suffered occasionally from pains, but they were of short duration, and had given her but slight uneasiness. On removing the plug, the placenta came away with it, accompanied by a few coagula of blood. There was very little flooding. It may be as well here to remark that the plug I used consisted of shreds of linen—the only available substance at hand.

This case I record as one of very many to illustrate a practice I am in the habit of pursuing in the treatment of cases of abortion in which the retention of the placenta is a source of trouble and anxiety to the accoucheur. Young practitioners especially have the notion that, at all risks the placenta should be removed before they leave the patient, and that she is in imminent jeopardy if it be permitted to remain longer than half-an-hour, or at the most three hours, and they are apt—too apt when nature does not assist them in this much-to-be-desired end—to summon to their assistance the ready resources of art so abundantly supplied them by those skilful and ingenious obstetricians whose mechanical inventions for this purpose are most beautifully and cunningly contrived, and who quite ignore the maxim that "meddlesome midwifery is bad." There are many such instruments described in the various works on midwifery. The best of the kind, I think, is the "wire crochet" of Dr. Dewees. With this we could not do any harm if we used the commonest precaution. But I maintain that, in the majority of cases, the finger of an expert practitioner and the *vis medicatrix naturæ* are more to be depended upon than any artificial appliance.

When the placenta can be felt over the os uteri the finger may be passed around it, and by a little skilful manœuvring it may readily be detached and extracted; but I would not advise any prolonged attempts to dislodge it, as by so doing the material parts may be much irritated and the patient's suffering greatly enhanced.

Plug and leave the case in the hands of nature. The kind of plug you should use is a matter of no very great moment so long as it is of a soft and yielding material, adapted to accommodate itself to the parts. In cases of emergency, where no time can with safety be wasted, you will be glad to avail yourselves of a good silk handkerchief, and this I have found to make an excellent tampon, answering every needful purpose. Cotton wool is, perhaps, preferable. Small pieces of sponge, about the size of a walnut, to which are attached pieces of string, are those more generally recommended. One great objection to them is, that when they become saturated with the fœtid discharge they become obnoxious to the patient, and are liable to promote the accession of febrile symptoms. To prevent this, they must not be kept too long in the vagina. Whatever plug we may employ, we should take care that the whole of the vagina be well filled, but not to such an extent as to cause by distension pain—(I may remark here that in midwifery operations we should always endeavour to avoid causing pain to the mother)—but so as to prevent the discharge of blood from the uterus externally. Some may be haunted by the idea that although the plug is *in situ* that internal hemorrhage may take place to an alarming extent, but I do not see how this can occur to any great degree when we bear in mind that there is a small uterus, not large enough to admit the hand, partly occupied by the placental mass, and the os uteri blocked up by the artificial plug. In my own practice, when I adopted the treatment I am describing, I have seldom met with hemorrhage that either caused me any anxiety or created much depression of the system of the mother. To recapitulate, I would recommend, in the treatment of cases of retained placenta in abortion, the following points to be observed:—1. If it be possible, remove the placenta by the finger; the sooner this is effected the better, but do not rashly act, and do not let the attempt last too long, as you have nothing to fear by its retention. 2. Plug, and use that which you have the readiest at hand, but the sponge plug, in my opinion, is the best. 3. Wait eight, ten, or twelve hours before you remove the plug, and you will generally find, on removing it, that the after-birth either comes away with it, or it is found lying in the vagina, from whence it may be easily withdrawn.—*Medical Times and Gazette, July* 14, 1866, *p.* 31.

64.—ON A CASE OF RUPTURE OF THE UTERUS DURING LABOUR.

By Dr. Thomas Dalton, Llandudno.

[The patient had been in labour five days, the membranes being broken that time, before Dr. Dalton was sent for. The pains were described as excessively strong and frequent until a short time before his arrival, when they suddenly ceased. She complained of feeling weak and sick ; but did not mention that she felt any peculiar pain or tearing sensation at the time of the cessation of the pains. The pulse was very quick and feeble, but this Dr. Dalton at first attributed to exhaustion from the long-continued and excessive uterine action.]

I found that the presentation consisted of the head and a foot, the head being in the right oblique diameter, and the foot opposite the left sacro-iliac synchondrosis. The foot was slightly in advance of the head ; *but both head and foot had entered the cavity of the pelvis, and were, in fact, quite down on the perineum.* There was a peculiar round tumour on the presenting part of the head, which felt larger and more elevated than an ordinary bloody tumour of the scalp. The pelvis was roomy, and the passages very dilatable. This led me to believe that the case was one of twins, in which both sets of membranes had ruptured, and parts of both children become engaged at the same time. This opinion I continued to hold as the most probable till the post-mortem examination proved that it was erroneous. There was a *very slight* bloody discharge from the vagina. As there seemed to be no reason why the labour should not be concluded naturally, I determined to wait for a short time in order to see if the pains would return ; but as the patient continued to feel faint and sick, I gave her a little gin (which was the only stimulant at hand) and warm water. This seemed to revive her a little. In about a quarter of an hour she said she felt a slight pain ; but, on examination, I found that it had no effect in advancing the child's head. At the same time I discovered that the foot had receded considerably, although the head was in the same position as before, and had not receded at all. There was still but very slight hemorrhage from the vagina. The patient now complained of a pain in the abdomen, which she could not accurately describe ; it was constant, and obviously not connected with true uterine contractions. She did not seem to rally, but, on the contrary, complained of feeling the faintness and sickness in a greater degree. These circumstances, together with the cessation of the pains, the slight discharge of blood, and the weakness and quickness of the pulse, led me to suspect that rupture of the uterus, to a greater or less extent, must have taken place. Having made up my mind on this point, I lost no time in despatching a messenger for assistance, as I considered that turning was impossible, and even, if possible, certainly unjustifiable. For the next half hour there was no great change in the patient's condition, and little or no retrocession of the presenting parts; but she complained of great and increasing abdominal pain. After this she became very restless, her hands and feet became cold, and the pulse almost imperceptible. I had a bottle of hot water put to her feet, and gave her a little hot gin-and-water every few minutes. She then seemed to rally a little; but about an hour after the despatch of the messenger, and just before his return, accompanied by Dr. Nicol, the poor woman suddenly fainted, and, in spite of all my efforts, died from syncope.

From the time at which I sent for Dr. Nicol to that of her death she never had a single pain, and the vaginal hemorrhage did not increase; in fact, the quantity of blood discharged externally during the whole labour could not have exceeded an ounce and a half, or at most two ounces. On further inquiry, the poor woman's husband told me that about ten weeks previously she stumbled in crossing a railway, and although she did not actually fall, she sustained a very severe shock; and that ever after that time she had complained of pain in the region of the uterus. During the last month of pregnancy her general health had been very indifferent; she was very timid, and often said that she did not think that she would get over her confinement, as she felt sure something was wrong, because her feelings were so very different from those she had usually experienced before delivery.

Autopsy.—On opening into the abdomen we found a quantity of serum and blood; there were also one or two adhesions anteriorly. The appearance presented by the uterus was peculiar, for at first sight it seemed very broad, with a sort of sulcus in the middle line of the fundus, as if it were to a certain extent double. On closer examination, we soon ascertained that the whole of the uterus was situated on the left side of the sulcus, and that the portion to the right consisted merely of the expanded peritoneal coat containing the child and the placenta, together with a quantity of blood. Strange to say, this peritoneal covering was to all appearance perfect, though there must have been a small opening in it posteriorly through which the blood escaped into the abdominal cavity. The laceration was posterior and almost longitudinal, extending upwards from the cervix; the fundus had remained partly contracted after the expulsion of the child into the abdomen; the lower part of the uterus, especially near the seat of the rupture, was very thin and flabby, and evidently in an unhealthy state. The child—for there was but one—was large and well developed; there was a very large bloody tumour of the scalp, plainly showing that the pressure exerted on it by the uterine contractions previous to the rupture must have been very great. The diagnosis with regard to the presentation, head and foot, proved to be entirely correct.

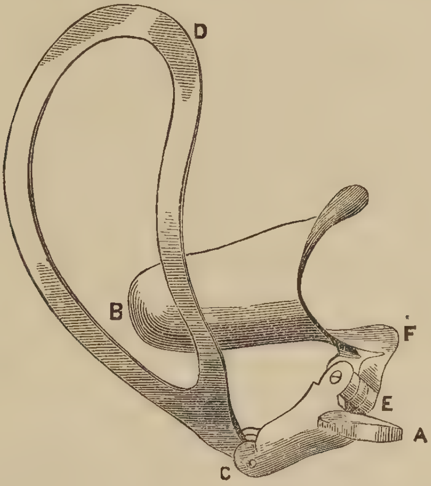
I forward the above report, as it seems to me that there are several features of interest in the case. In the first place, *any* case of rupture of the uterus must possess a certain amount of interest from the rarity of the accident. Secondly, the presentation was very peculiar, and, as far as I am aware, very rare; for, be it remembered, the head and foot not only became engaged together, but they had actually been so forced quite down on the perineum before the rupture took place. Again, the negative character of the symptoms in this case is worthy of note; for, whilst it is evident that the rupture must have occurred before my arrival at the house, there was, I think, nothing, in the first instance, to lead me to suspect such a frightful accident. —*Lancet, July* 21, 1866, *p.* 64.

65.—EMMET'S PERINEAL RETRACTOR FOR VAGINAL EXAMINATIONS.

Dr. Emmet, Surgeon to the Woman's Hospital of New York has furnished us with the following description of his speculum:—

"This instrument brings the neck of the uterus into view, as with Sims's speculum, by retraction of the perineum, but with the advantage that an assistant is unnecessary. The patient should be placed in the same position on the left side, the lower limbs flexed well on the abdomen, with the upper or right leg in advance of the under one; the lower, or left arm, withdrawn from under the body, and flexed across the back, so as to rotate the chest as flat as possible on the table or operating chair. The proper position is absolutely necessary in the use of both instruments; and without it is observed in detail, a great advantage is lost. When properly applied, either will expose a larger portion of the vagina than can be done by any other instrument in use.

"After separating the instrument fully, the fenestrated blade, *D*, is turned up as represented in the cut; the vaginal portion is then introduced on the index finger of the right hand, at the same time the perineum is pushed backwards, and the instrument held firmly in position, while the thumb-screw, *A*, is turned by the other hand, until the instrument is adjusted. By the thumb-screw, the point, *B*, of the speculum is carried into the hollow of the sacrum; with the joint at *C*, the blade *D* can be adjusted on the right buttock; by carrying it in the direction *D*, as the lower portion of the instrument in the line *C*, *E*, *F* rests on the lower buttock, along the sulcus, and the vaginal portion is in the shape of a cone, the upper labium of the vagina is elevated. If the cervix uteri is not brought into view at once, a depressor may be needed to push aside the anterior wall, although as a rule it is not necessary. By elevating slightly the instrument at *F*, the point *B* becomes depressed so as to bring the neck often into view, when not presenting. As it is impossible to have a single blade of a size to answer for every case, as with Sims's speculum, a little manipulation is frequently needed when the vagina is unusually long or its wall relaxed; a depressor is therefore required; when once the neck is brought into view, it should be seized by a tenaculum, and drawn in advance of the fold, and it generally remains in position afterwards. The tenaculum may remain hooked into the cervix, as it is absolutely necessary to steady the organ properly during any manipulation, even when the neck is brought into view at once.



"I have used this instrument for the treatment of uterine disease, and for different surgical operations on the base of the bladder, with the greatest satisfaction. Having an experienced female assistant, who has been long accustomed to the use of Sims's speculum, I shall continue to use his instrument to a great extent in my office practice, from the fact that it is applied as soon as introduced. But for an examination without the aid of an assistant, and for long operations, this instrument, when properly applied, affords so great a facility, that I believe, after one has become familiar with its use, it will take the place of all others. It gives no pain, from the fact that the perineum is compressed to one side of the coccyx; and from the the steadiness with which the retraction is maintained, I have found that this instrument is preferred to the jerking and unsteady handling of Sims's speculum by an assistant unaccustomed to its use.

"I am greatly indebted to the skill and perseverance of Ford & Co., of Fulton-street, for putting into practice the principle, after many disappointments and modifications of the mechanical application. They have so constructed the instrument that one of several blades of different sizes can be attached at the joint *E* when required, although an average size has been fixed upon as the standard, which will answer for nearly every case by a little dexterity on the part of the operator."—*New York Medical Record, May* 1, 1866, *p.* 115.

66.—ON THE RELATION OF PHLEBITIS AND THROMBOSIS TO PYÆMIA.

By William S. Savory, Esq., F.R.S., Assistant-Surgeon to St. Bartholomew's Hospital.

[Formerly, the abscesses which were discovered after death from pyæmia, or detected during life, were regarded as collections of pus which had been merely transferred as such by the circulation from the original source to the structures in which they were afterwards found. Cruveilhier, however, afterwards proved that these secondary abscesses are not simply passive deposits of pus, but that the pus is formed in the situation in which it is found.]

Arnott, in his masterly essay in the Medical and Chirurgical Transactions, after referring the formation of the abscesses in the lungs, liver, and joints, which follow injuries of the head, parturition, great surgical operations, and suppurating wounds, to inflammation of the veins of the part primarily affected and the entrance of pus into the circulation, says, it becomes a question whether the occurrence of phlebitis, and the passage of pus from an inflamed vein into the circulation, are not sufficient of themselves to account for the secondary affections of wounds, without its being necessary to resort to an absorption of the same fluid from their suppurating surfaces.

For some time, however, it was believed that the symptoms of what is now called pyæmia, were due to the extension of inflammation along the veins to the heart.

In some cases of amputation the veins; in others, the arteries; and in others, again, both the veins and arteries, will be found inflamed, from the point of the stump to the very auricle and ventricle, and in many parts lined with coagulable lymph, or filled with purulent matters, to various distances.

When a vein is wounded, the inflammation, which is the effect of the injury, sometimes extends along the lining of the vessel into the principal venous trunks, and in some instances even to the membrane which lines the cavities of the heart.

Hunter wrote, "it may either be that the inflammation extends itself to the heart, or that the matter secreted from the inside of the vein passes along that tube in considerable quantity to the heart, and mixes with the blood."

He says farther on, "in all cases where inflammation of veins runs high, or extends itself considerably, it is to be expected that the whole system will be affected. For the most part the same kind of affection takes place which arises from other inflammations, with this exception, that where no adhesions of the sides of the veins are formed, or where such adhesions are incomplete, pus passing into the circulation may add to the general disorder, and even render it fatal."

Mr. Arnott showed the fallacy of the doctrine just alluded to, and argued "that death in cases of phlebitis does not take place from inflammation extending to the heart, but that the entrance of pus or even of some other product of inflammation from the inflamed part of the vein into the circulation is the source of the alarming and fatal indisposition."

Cruveilhier "argued that the first effect of all phlebitis is a coagulation of the blood with adherence to the walls of the vessel."

And again, that the deposit of "pus" is not between the vein and the clot formed in its interior, but in the very centre of the latter. But he gave a fanciful explanation of this occurrence.

Another fact pointed out by Cruveilhier is that no ordinary mark of inflammation is ever noticed upon the inner surface of the vein in the various stages of phlebitis, that is to say, no injection of capillary vessels can be discovered. The deep red colour is a stain due to imbibition.

Now pyæmia is often witnessed without any evidence whatever of phlebitis. The worst forms of phlebitis often exist without producing pyæmia. Indeed it is very rare to see pyæmia following upon well marked phlebitis during life. Pyæmia is rarely preceded by any obvious affection of the veins.

"It is quite the exception," says Dr. Wilks, "to discover phlebitis as a cause of infection." Again, "amongst the various causes of pyæmia, phlebitis may take its place, although it is very remarkable how seldom the veins can be found affected." And again, "so far from phlebitis being a cause of pyæmia, it is remarkable how often the former occurs without any contamination of the blood whatever; that is, if we call that phlebitis where we find a vein and its branches quite closed by coagulum or adherent fibrine."

Indeed all agree now that pyæmia often occurs without the least evidence of any affection whatever of the veins.

It must of course be admitted that when after death from pyæmia no phlebitis can be discovered, it may nevertheless exist, and escape detection. But this argument has been unduly pressed into the service of those who regard pyæmia as always consequent on phlebitis or some affection of the veins. This objection has been repeatedly urged against the fact that pyæmia often exists when no traces of inflammation of a vein can be discovered. But before any weight can be given to this plea, the connexion between pyæmia and phlebitis, or even thrombosis, must be established upon better evidence than that by which it has been hitherto supported.

Then Gulliver demonstrated the important fact that what was regarded as pus in the interior of the veins was, although a puriform, not a purulent fluid. The experiments and observations recorded in his admirable paper led him to the following conclusions:

"1. That conagulated fibrine, when removed from the body and subjected to a blood heat, commences to soften in about forty hours, assuming the colour and consistency of pus, but easily distinguishable from it by microscopic and chemical examination.

"2. That the purulent-like fluid found in the fibrinous clots of the heart and arteries, and so frequently in the veins, is essentially distinct from pus, and analogous to, if not identical with, softened fibrine.

"3. That the softening of coagulated fibrine is an elementary pathological condition of frequent occurrence, distinct from suppuration, and constituting a considerable proportion of the cases generally denominated suppurative phlebitis."

Dupuytren, but after Gulliver, remarked that the so-called pus in the veins may be due to an alteration effected after death. The greyish clots and puriform appearance may be but an effect of stasis of the blood, of fever during life and of the temperature which remains some time after death.

Veins may inflame. But Virchow and others have shown that phlebitis does not lead to pus in the canal of the veins; nor even, it is affirmed, always to coagula.

Phlebitis "is an inflammation which really affects the walls, and not the contents of a vessel. In the larger vessels the most different layers of their walls may become inflamed, and enter upon every possible phase of inflammation, and yet all the while their channel may remain entirely unaltered." "Hitherto no experimenter who carefully prevented the blood from streaming into the vessels has succeeded in producing an exudation which was deposited in their cavity. On the contrary, when the wall is inflamed, the exuded matter passes into the wall, which becomes thicker, cloudy, and subsequently begins to suppurate. Nay, even abscesses may form, which cause the walls to bulge on both sides like a variolous pustule, without any coagulation of the blood ensuing in the cavity of the vessel. At other times, certainly, phlebitis, properly so-called (and in like manner arteritis and endo-carditis), is the cause of thrombosis, in consequence of the formation of inequalities, elevations, depressions, and even ulcerations upon the inner wall, which favour the production of the thrombus. Still, whenever phlebitis, in the usual sense of the word, takes place, the alteration in the coat of the vessel is almost always a secondary one, and indeed occurs at a comparatively late period."

That the fibrinous deposit so often found upon the lining membrane of veins is from the blood, and not lymph from the walls, seems proved by the following facts, which may be repeated here.

It never occurs in a part from which blood is excluded; and lymph has never been produced upon the lining membrane of a vein in any experiment when blood was excluded from the canal.

It can be detached, for the most part, with facility from the lining membrane, which then presents a natural aspect or is at least only somewhat blood-stained.

Such deposits of fibrine are often continuous with undoubted coagula, which have not yet lost their colour.

Thus the old idea that lymph may be poured out from the lining membrane of a vein, and that pus may be formed in the canal, depended upon a misinterpretation of what was observed in veins which had been plugged by coagula. When clots form in veins they, after awhile, gradually lose their colour, and usually at length disintegrate. Such disintegration, advancing most rapidly in the centre of the clot, gives rise to a granular and oily matter which is puriform to the naked eye, while around there is a shell of fibrine presenting the characters of ordinary lymph.

There is much evidence, but of a very contradictory nature, concerning the existence of pus in veins. However, it may be affirmed that it is not yet established that pus is ever found in veins except under peculiar and extraordinary circumstances, such as those already alluded to. Just as the evidence of inflammation of the lining membrane of veins, such as the presence of effused lymph, has broken down under critical examination, so the presence of pus in the interior of a vein becomes more rare and exceptional the more thoroughly the subject is investigated.

But Virchow and Kirkes have shown how fragments of fibrine, being detached from the cardiac valves or from the interior of vessels where they had formed, or becoming disintegrated in the manner described by Gulliver, may pass into the circulation, be transported to distant parts, and at length, according to their size, block up arteries or capillaries. To this process of obstruction of remote vessels by fragments which have been detached from a disintegrating thrombus, or clot formed in a vein, Virchow gave the name of *embolia.*

Thus arose the great doctrine of the cause of the secondary deposits of pyæmia, which Virchow has expounded with such masterly skill. According to this, the metastatic deposits have their origin in embolia. A clot, which has formed in some vein, softens and crumbles away, or minute particles are detached from the surface of a cardiac valve. The granules, thus set free, are carried to distant parts, and at length block up the finer vessels. Thus, mechanical obstruction is the starting-point of the mischief. In explanation of the small and very numerous deposits which are often found, Virchow goes farther, and declares it to be a necessary inference that, when a considerable fragment of a thrombus becomes wedged at a certain point in an artery, it may, in its turn, crumble away through the onward pressure of the blood, and thus the minute particles to which this crumbling of the larger plug gives rise be conveyed into the small branches into which the vessel breaks up.

But clots in veins often occur, and softened ones too, without the occurrence of pyæmia. "It may be observed," said Gulliver in the paper referred to, "that the softening of fibrine certainly occurs sometimes without appreciable suppuration in any part of the body. This, however, constitutes no fatal objection to Virchow's doctrine, for in such cases conditions may, and often do, exist which prevent the passage of the granules of fibrine into the circulation. But not only may softened clots exist without pyæmia, but, what is more to the point, pyæmia may occur without any evidence whatever of the existence of softened clots.

And when in pyæmia the veins are found affected and filled with coagula, in what relation does this stand to pyæmia? May not both be effects of a common cause—of the absorption of poison which at once contaminates the blood and causes its coagulation in the nearest veins through which the poison first passes, perhaps inflaming, or at least irritating, their walls? May it not be that thrombi, and even phlebitis, are the local, and "pyæmia" the general, effect of the passage of the morbid poison into the blood? But neither phlebitis nor thrombi, nor any affection of the veins whatever, is a necessary link in the production of pyæmia.

Thus, as the relation formerly supposed to exist between phlebitis and pyæmia was set aside because there is no evidence to support it, so the relation of thrombosis—which has been substituted for phlebitis—to pyæmia is, in its turn, open to at least this objection, that many cases of pyæmia occur which cannot fairly be referred to this cause; many even in which there is no evidence to show, and no reason to believe, that they have been preceded by the formation and softening of any clot.

In the present state of our knowledge, then, the relation in which an affection of the veins stands to the pyæmia appears to be this.

They may be throughout perfectly healthy. For it will not do to argue that when, in pyæmia, no morbid condition whatever of any vein can be discovered, that nevertheless it exists, and escapes detection. This is possible, of course; but in the absence of evidence to support it, with the proof that such a condition is by no means necessary to the event, and that all the effects witnessed in pyæmia may be otherwise produced, such a doctrine has no claim to acceptance.

The veins may be inflamed. Now the only acceptable explanation of the way in which pus—as pus—can pass into the circulation, except in extremely rare and exceptional instances, is that it is formed in the interior of veins as a consequence of phlebitis. Yet not only do cases of pyæmia occur without the slightest evidence of phebitis, but—assuming that our knowledge of phlebitis is still very defective, and that the present views need some qualification—evidence is wanting to show that in phlebitis any matter is added to the blood, or any change induced in it which causes pyæmia. Yet it is quite possible or probable that the veins, when inflamed, may, like other inflamed structures, furnish some morbid matter which affects the blood.

One or more veins may be occupied or filled with coagula more or less adherent, and in various states; firm and solid throughout, or softened, or even diffluent. But, in the majority at least of cases of pyæmia, there is no evidence of the existence of such softened clots, or that such matter has passed into the circulation.

[Mr. Savory then passes on to the question as to the relation between thrombosis and phlebitis. The specimens in the museums of the London hospitals prove that phlebitis never occurs without some degree of plugging or deposit of fibrine in the canal of the vessel, but that on the other hand a vein may be plugged without such previous alteration of structure as is attributed to phlebitis. In examining the vein after death, we find both alteration in its structure and fibrinous deposit in its interior.]

Which occurs first—the changes in the wall of the vein or the coagulation of the blood in its canal?

In some cases unquestionably the coagulation, for it sometimes happens that a vein, which previously gave no sign whatever of being in any way unhealthy, will suddenly become plugged throughout, and at this time the most careful examination can detect no change whatever in the walls of the vessel. In such a case the clot may shrink or disappear, and the vein may resume its natural condition, no traces of the previous mischief remaining, or there may ensue upon the thrombosis those symptoms of phlebitis and those changes which lead to the alterations of structure just alluded to.

On the other hand, but these cases are undoubtedly more rare, the symptoms of phlebitis seem to precede the formation of a clot in the canal. For some time after pain and tenderness have existed in the course of a vein, evidence may be obtained, by pressure and otherwise, that the blood still flows through it. If the mischief progresses, however, this evidence soon disappears.

Thrombosis, then, without phlebitis, is evidently of more frequent occurrence than phlebitis without thrombosis.

But if either may exist without the other, what is the cause of thrombosis?

It must be confessed that thrombosis sometimes occurs, and these are the most striking cases, under circumstances in which the cause of it is altogether a mystery. When clots form in portions of veins that have been torn or bruised, or otherwise damaged, we may explain the mode in which they are produced. When they arise in veins which communicate with wounds or diseased structures, or even in veins which are compressed so that their current is interrupted, we may still assign a cause for the result. In an altered condition of the venous channel, in some abnormal state of the blood passing through it, or in some obstruction to its flow, we may perhaps discern something of the cause which determines its coagulation. But in our museums specimens may be found of veins which are plugged with clot, the coats of which are nevertheless apparently quite healthy.

If, then, we conclude that healthy vessels may become plugged, we must seek elsewhere than in the vein for the cause of the coagulation. But it may be that incipient changes in the lining membrane of the veins, which may not yet have advanced sufficiently to be observed, may have still determined the coagulation of the blood. It may be so when the coagulum is recent. But this idea is hardly tenable against the fact that there are specimens of veins which contain old clot, clots which have existed long enough to undergo the usual changes in vessels, the walls of which are still free from any visible morbid change. If the formation of clot had been determined in these cases by the morbid state of the vein, it is reasonable to believe that during the time the clot has been undergoing the ordinary changes the disease of the vessel would have become apparent. From the consideration of these latter cases, then, the conclusion is drawn, not only that clots may form in healthy veins, and that, therefore, in some cases at least, the cause of the coagulation must be independent of any disease of the vessels themselves, but that the existence of a clot in the canal does not necessarily provoke disease in the walls of the vessel.

Now when a certain vein, or series of veins, apparently in all their integrity, communicating with no diseased or injured part, and carrying blood like that which circulates through neighbouring and other veins—when a set of veins under these circumstances becomes almost suddenly filled thoughout with clot, where, in what direction, are we to look for the cause? Is it in the vessels or in the blood? The vessels have given no previous evidence whatever of any morbid condition; nor, perhaps, will any be discerned afterwards by the most minute scrutiny. No local disease exists, nor has any injury been sustained. And let it be observed that, even for the moment assuming one of these, it will not account for the extent of the clot, for coagula formed in such cases are limited in their extent. Again, if the blood be in fault, why is the effect so local, limited to certain veins, or, indeed, to the veins at all? for it would appear, as a rule at least, that arterial blood deposits fibrine more readily than venous. The assumption of some spot of mischief as a starting-point would not go far enough; the assumption of some affection of the blood would seem to prove too much.

[Many suggestions have been made as to the mode of arrest of circulation in the vein. These, however, only show how very far we are at present from any knowledge of the pathology of thrombosis.]

Still in the midst of the darkness may be found, perhaps, some clue to the direction from which light may be hereafter thrown upon the subject.

However much the formation of clots in veins may be influenced by the quality of the blood and the rate of its movement, the determining or essential cause must surely be, in many cases, a local one; otherwise it would seem that coagulation ought to be more general, and not limited to this or that particular vein. It might be, on the one hand, that such local cause would often prove inoperative unless assisted by an increased disposition to coagulate on the part of the blood, and yet, on the other hand, if sufficiently intense, it will speedily induce the formation of a clot, be the condition of the blood what it may. Thus thrombosis appears to be the almost inevitable result of certain changes in the walls of the veins, and I do not remember to have seen a vein which was thickened and indurated, and exhibiting the changes usually attributed to phlebitis, the canal of which was nevertheless free from clot. That the disposition of the blood to coagulate is powerfully influenced by the nature of the surface with which it is brought in contact, seems to be a well-established fact. It was pointed out by Hunter, has been more or less clearly recognised ever since, and recently more fully demonstrated by the experiments of Lister. Thus, whatever may be the cause of it, there is a striking difference in this respect between dead and living parts, and living parts themselves possess different degrees of influence. So Hunter, after pointing out that "the want of disposition to coagulate whilst moving in the living vessel does not arise from the motion," goes on to say, "This harmony of the blood with the solids is more observable in some parts than others. The parts with which it is in the greatest harmony are the vessels; this is evident from its retaining its fluidity longer in contact with them without motion than in any other part of the body, though equally enclosed in living parts." But this, of course, applies to the blood and the vessels only in a natural and perfectly healthy state, and it is not difficult to believe that any deviation from this—whereby the harmony is disturbed—must proportionately tend to determine the formation of clot.

With reference to the cause of thrombosis, and more especially to the tendency of the blood itself to coagulate, it should be observed that the clots found in the interior of veins may fairly be divided into two classes. In one, the clot is a simple solid cord, and presents on section an uniform structureless surface. In the other, the clot is distinctly laminated, and a section shows a number of concentric layers. This difference, which is equivalent to that observed in the interior of aneurismal sacs, must imply a difference in the mode in which the clot was formed. In the first case, it would seem as if the whole of the blood with which a vein is filled had throughout coagulated, the vessel subsequently contracting upon and adapting itself to the clot as it shrunk. In the second, it would seem as if that portion only of the blood in direct contact with the wall of the vessel had deposited a layer of fibrine, and that within this another had afterwards formed, and so on. This view is strengthened by what is often observed in the examination of these cases. In recent ones, sometimes the whole canal of a distended vein is found filled with a large dark soft uniform clot. In other cases, perhaps old ones, when the canal of a large vein is laid open, it may, at the first glance, appear of its natural size, and little or no thickening of the wall may be observed, but more minute observation shows that upon the lining membrane is deposited a thin layer of fibrine. Further, it is to be observed, what from this point of view would be anticipated, that when the contents of veins of about the same normal diameter are compared, those clots which are formed of concentric layers usually far exceed in calibre those which are uniform throughout.

But to conclude, the sum of all that has been said amounts to this—

Thrombosis may exist without any evidence of phlebitis, and very often occurs without being followed by pyæmia.

Phlebitis may occasionally exist without thrombosis, and often occurs without being followed by pyæmia.

Pyæmia often exists without any evidence of thrombosis or phlebitis; still oftener it occurs without any evidence whatever that it has been preceded by either of these, or of any other affection of the veins.

It has therefore not been satisfactorily shown that either phlebitis or thrombosis stands, in any especial or peculiar manner, in relation to pyæmia, as cause and effect.—*St. Bartholomew's Hospital Reports, Vol. II.*, 1866, *p.* 46.

67.—ON THE DIFFERENT MODES OF DEALING WITH THE PEDICLE IN OVARIOTOMY.

By T. Spencer Wells, Esq., Surgeon to Her Majesty's Household, &c.

[A great number of plans have been proposed for dealing with the pedicle in cases of ovariotomy. Mr. Wells performed two cases of ovariotomy at Chester, during the meeting of the British Association there last year; in both he used only the clamp. He observes:]

If a pedicle be small enough to be securely held in a clamp of moderate size, and long enough to permit of the clamp being fixed outside the closed wound without much pull on the uterus or broad ligament, I wish for no readier or more successful method. The objections to it are either groundless or trivial. It is said to be very painful; but I have seen a good deal of pull with very little pain, and much more severe pain in cases where the ligature was used than I ever saw in clamp cases. So with sickness: I have seen as much, or more, after the ligature or cautery, as I ever saw after the clamp. It is said to set up fœtid discharge and poison the wound or the patient; and so it does if proper care be not taken. But if the strangulated part of the pedicle which projects beyond the clamp be well saturated with perchloride of iron, as you saw me use it just now, the slough is tanned; it becomes as hard and dry as a piece of leather, and there is an end to that objection. It is said to cause suppuration about the wound; but this, again, I have seen quite as frequently, in proportion, after the ligature or cautery. I never saw more profuse suppuration of the stitches than in one case where I divided the pedicle with the *écraseur*, and closed the wound with platinum wire sutures. Then, after the wound is closed, it is said to lead to a re-opening each month, and an escape of some menstrual fluid. And this is true in some—perhaps in nearly a third—of the cases. But if the patient be prepared for it, it is not of the slightest consequence. The Fallopian tube contracts completely after a few months, and there is no further escape. The fact that it does escape sometimes is to my mind an argument in favour of the clamp; for if menstrual fluid can escape through the partially closed Fallopian tube fixed in the cicatrised wound, so it may escape if the tube be left within the peritoneal cavity, and the result may be a fatal hematocele. I have known this to occur in cases where the ligature was used and cut off short; and I believe it to be one of the strongest objections to this method, or any intraperitoneal method of dealing with the pedicle. As to any fancied impediment to the increase of the uterus in pregnancy, and to its contraction during labour, from the adhesion of the tube to the cicatrix, I can only say that nine of my patients have had children after ovariotomy—two of them two children—and there was no such complaint in any one case. One *real* objection to the clamp is that it may possibly pull on intestine, or a tense pedicle may strangulate intestine (and I have seen one such case). But this objection is of little weight if the use of the clamp be restricted to cases where the pedicle is so long that there is not much drag on the clamp. In such cases, I repeat, I desire no better method. But where we have a broad, thick, short pedicle, or a broad connection between uterus and cyst rather than a distinct pedicle, we want something better than the clamp. And we have the choice between wire or needle pressure, the ligature, the *écraseur*, and the combination of crushing and cauterisation, to which I have before alluded as an improvement due to Mr. Clay, for which he has certainly not received due credit.

I say nothing about acupressure or the wire compress, because I have never tried them. Sir James Simpson was successful in one case, and the plan is certainly worthy of trial.

The *ligature* of the pedicle can always be effected by transfixing it, and tying in two or more portions, before the cyst is cut away. Or a clamp may be first applied, in the cyst cut away, and the pedicle then transfixed and tied below the clamp. But, if this be done, the clamp must be loosened before the ligatures are tightened, or the compressed tissues are so held that the knot cannot be tied so tight that it will not slip off as soon as the clamp is removed. If it be desired only to tie the vessels, it may be done by feeling the arteries, and carrying a ligature round them through the pedicle before the cyst is cut away; or, after the application of a clamp and removal of the cyst, holding the pedicle carefully with forceps as the clamp is loosened, and tying any vessel which bleeds. The great objection to this plan is, that there is often much loose cellular tissue, rich in small veins, which go on oozing after all the larger vessels have been tied. Whichever may be the plan preferred, the important question arises, Shall the ends of the ligatures be cut off, and the wound closed? or shall they be left hanging out through a part of the wound, purposely left open for their passage, and that of the slough they embrace when it separates? Dr. Clay of Manchester still advocates this latter practice. I have tried it, and with success in about a fifth of the cases only; and I shall not willingly adopt it again. In its favour, it may be said, that it is a method applicable in all cases; that it secures an outlet for serum from the peritoneal cavity; and that, after the separation of the ligature and slough, no foreign body is left within the patient. But it seems to me better to have a choice of methods, and adopt each in its appropriate case, than to strive after one method applicable to all cases. I think the ligature-threads act as a sort of seton in in the peritoneal cavity, set up inflammation, and excite the formation of the serum for which they are said to provide the outlet. Then, if the patient recover (and I have very great doubt whether very many subjected to this plan do really recover), there is a great liability to ventral hernia. The cicatrix remains weak at the spot where the ligatures passed out, and it yields before the pressure outwards of the viscera. I have seen this in nearly every case where I followed this plan; but I do not remember more than two cases where it followed the clamp. Therefore, if we use one or more ligatures, I am inclined to cut off the ends short, and close up the wound completely. Wire has been used for this purpose; but it seems an irrational practice. Silk, if pure, is an animal substance; and experiment proves that it may be absorbed. Wire cannot be absorbed, and must be more or less of a mechanical irritant. I tried wire on one side and silk on the other side of a sheep on which Professor Gamgee operated for me at the Albert Veterinary College, and the superiority of the silk was manifest. But what we have to look to is the effect on the tissues strangulated, rather than the material by which the strangulation is effected. If anything like what goes on outside the body when the clamp is used, or inside when the wound is left open for ligatures, were to go on when the wound is closed, it is difficult to understand how any patient could possibly survive the process. She would almost infallibly be poisoned by absorption of the fœtid products of the decomposing stump. But a very different series of changes must go on when the wound is closed and access of air is shut off. At any rate, experience proves that patients do survive the process; and *post-mortem* examination has shown that ligature and pedicle have been coated by a sort of capsule of lymph. In my own hands, this practice has been much less successful than the clamp; and, even when patients have recovered, some of them have long remained in a state of semi-invalidism, very different from the robust health which is the rule after successful clamp-cases. This plan is that always followed by Dr. Tyler Smith. It was originated in 1821, by Dr. Nathan Smith, of Baltimore, who used *leather* ligatures. Dr. Rogers, of New York, in 1830, also cut off his ligatures "close to the knot, and then left them to absorption." If I use the ligature, I feel disposed to cut off the ends whenever the patient is in pretty good condition, and sthenic peritonitis with effusion of lymph may be expected; but if low diffuse peritonitis and effusion of serum may be feared, then I suspect it would be better to leave the ends of the ligatures, and secure a drain through the wound of the serum. But we should still search for a better method than the ligature.

The *écraseur* I used once, and successfully. But I have not ventured on it again; for, if it should prove untrustworthy, and internal bleeding occur in any case, one's self-reproach would be very painful.

The *cautery* alone would almost certainly fail to stop such large vessels as are frequently met with in a pedicle. So might the *écraseur* alone, or the crushing which precedes division by the *écraseur*. But the combination of crushing and the cautery is certainly efficacious in a considerable proportion of cases. Mr. Clay of Birmingham, as I said just now, introduced the practice, and carried it out by his "adhesive clam" and hot irons. I wrote to him at the time, that, if it answered for adhesions and omentum, it ought to answer for the pedicle. And I might have tried it; but my first trial on a piece of omentum was unsuccessful, and I did not repeat it. But latterly Mr. Baker Brown has published so many cases in which he has so successfully secured the pedicle on Mr. Clay's principle of combining pressure with the cautery, that I have tried it in five cases. Three of the patients recovered, and two died. In three, the cautery was alone sufficient to stop all bleeding. Two of these patients recovered, and one died. In two others, on opening the clamp, considerable vessels bled, and ligatures had to be applied. One of them recovered and the other died.—*British Medical Journal, Oct.* 6, 1866, *p.* 378.

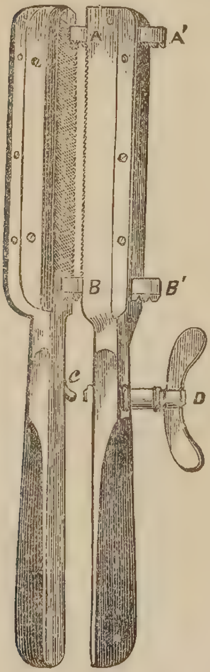
68.—ON THE TREATMENT OF THE PEDICLE OF OVARIAN TUMOURS BY THE ACTUAL CAUTERY.

By I. B. Brown, Esq., London.

Mr. Baker Brown read a paper (at the Meeting of the British Medical Association at Chester last year) on the treatment of the pedicle of ovarian tumours by actual cautery. This practice has been adopted by the author in thirty-six cases, twenty-three of which had previously been given to the profession in two papers read before the Obstetrical Society. Mr. Brown now gave in detail thirteen more cases. The following analysis will show the result of this treatment:—Of the whole number, five have died, of which two occurred in the first twelve and three in the present series. In not one of these had death resulted where the cautery had been used alone, with the exception of the second; here death was due to hemorrhage from the site of an adhesion in the utero-rectal fold, which could not safely be reached by the actual cautery. In the remaining four, one or more ligatures had been used in addition to the cautery, the latter, from various causes, not having perfectly secured the pedicle. In these four, the causes of death were respectively—1. Peritonitis, with hypertrophied heart and thickening of aortic valves. 3. Peritonitis; no autopsy allowed. 4. General peritonitis. 5. Shock; a small quantity of coagulated blood on the stump. Mr. Baker Brown drew the following conclusion from his experience of this treatment:—That it is preferable in all cases first to employ the cautery. Should this fail, no harm has been done, and the ligature may be resorted to without disadvantage. The method of using the clamp was fully explained, and a newly-improved instrument was exhibited. This clamp possessed parallel blades, and the bone, formerly fixed to the back of the clamp to diminish heat during division, was now eparated, except by two long rivets, from the blades.—*Medical Times and Gazette, Sept.* 1, 1866, *p.* 229.

69.—ACTUAL CAUTERY PARALLEL CLAMP.

The accompanying wood-cut depicts a new clamp, designed by Mr. Thomas Chambers, senior assistant-surgeon to the London Surgical Home for Diseases of Women, &c. This instrument has several advantages over those now in use.



1st. Its great simplicity, and the perfect ease with which it can be applied and removed.

2nd. The instruments now in use, acting like a pair of scissors, press unequally on the pedicle while the blades are being brought together; whereas, with this instrument, the blades being perfectly parallel, the pressure is applied along the whole length of the blades at the same moment of time, and with the greatest accuracy and nicety.

3rd. Being a right-lined quadrilateral figure whose opposite sides are parallel and equal, this clamp can receive and completely compress a larger pedicle than any other instrument of the kind. It has been used by Mr. Holmes Coote as well as by Mr. Chambers, and was found to act admirably. In addition to its value in the treatment of ovarian pedicles, this clamp may be advantageously used in cases of omental hernia where it it found necessary to remove a portion of the omentum; also in castration, hemorrhoids, vascular polypus, &c., &c.

The clamp consists of two parallel blades, held together by two racks (A and B). In these racks a pinion works which is moved by the handle (D). Every half revolution of this handle moves the upper blade one-sixteenth of an inch. When the trigger (C) is pulled back by the index-finger of the left hand, the blades of the instrument are immediately connected or disconnected at the will of the operator or his assistants.

The clamp is made by Messrs. Mayer and Co., of Great Portland-street.—*Lancet, July* 24, 1866, *p.* 71.

70.—ON FIBROUS TUMOURS OF THE UTERUS.

By Dr. R. T. Tracy, Lecturer on Obstetrics in the University of Melbourne, and Honorary Physician to the Lying-in-Hospital.

[The patient was thirty-three years of age, and had had two children, both still-born; the last delivery being ten years ago. She had enjoyed good health until nine months before, when the menstrual discharge begun to be too profuse, on several occasions amounting to severe flooding.]

On making a digital examination the os was found to be healthy, the cervix short, and somewhat more patulous than in health. The uterine sound was introduced, and a distinct tumour found to be imbedded in the anterior wall of the uterus. The sound could be felt passing over the bulging portion of the tumour as it encroached on the cavity of the uterus, and passed in to four and a half inches. Absolute rest in the recumbent position was ordered, enemata of cold water daily, and a supporting diet. After a few days of this treatment I introduced one or two large sponge tents in succession, in order to enable me more fully to explore the cavity of the uterus, so that I might ascertain whether the tumour was attached by a pedicle round which I might put the cord of an ecraseur. After full examination, it proved to be imbedded in the wall without an intervening pedicle. I therefore determined to freely incise the os and cervix, in order to follow out the plan noted in my last paper, viz., to equalize the pressure of the uterine fibres on the tumour and lessen its vascular supply.

On May 28th, I freely slit up the os and cervix with my double uterotome, and, after the operation, continued the use of ten-drop doses of liquor secalis cornuti (B.P.), which I should mention had been given three times a day from the time the os and cervix had been opened by sponge tents.

In a few days a purulent discharge began to come away freely. Not a drop of blood was lost by the patient after the hemorrhage consequent on performing the operation had ceased. *The hemorrhage which had been almost incessant, and which was fast wearing the patient out, stopped entirely after the uterotome was used.* A smart rigor occurred on the third day, and the purulent discharge became very offensive. There was tenderness over the lower part of the abdomen, and great irritability of the stomach. Brandy was given pretty freely, linseed meal poultices applied over the abdomen, and opiates were administered at night; warm water injections to the vagina used frequently.

On the 6th June, a digital examination discovered a slight protrusion of the sloughing tumour at the os. The purulent discharge was still very profuse.

On the 14th of June, a mass of sloughing fibrous tumour, about the size of a large hen's egg, was expelled, and speedily all the serious symptoms subsided. The pulse which had been up to 120, soon fell to a natural rate, the sickness of the stomach and the tenderness of the abdomen speedily passed away, and the patient made an excellent recovery. She left the hospital on August the 10th, quite well.

I am inclined to believe that in some of my examinations of this case with the uterine sound, I broke into the envelope of the tumour, and caused the more rapid sloughing of the mass. No doubt this aggravated the constitutional disturbances, and, to a certain extent, caused the case to resemble those in which a piece of the tumour is gouged out, and in which pyæmia is too often apt to occur. The vitality of these tumours is so low that very little interference suffices to induce suppuration in the structure.

Case 2.—E. S., aged 45 years, was admitted November 15th, 1865. She had been many years married, but had never been pregnant. She stated that for the last two years she had suffered from severe bearing down pains in the pack and abdomen, and had had frequent and severe hemorrhages from the womb. The abdomen had been considerably enlarged for many months. She described the loss of blood to have been enormous at times, and this statement was borne out by her appearance on admission. She was a largely-built, masculine-looking woman, but presented a most intensely anæmic appearance, the skin was of a greenish-white colour, exactly like the appearance presented by a patient suffering from malignant disease.

On examination, however, the os and cervix were found to be perfectly healthy, but flabby and patulous. The abdomen was enlarged to the size of that of a woman four or five months pregnant, and on introducing the sound it passed to six and a-half inches.

After careful consideration, and the cautious use of the sound, taking care not to press the point against the tumour, as in the last case, I diagnosed this to be one of non-pediculated fibrous tumour, and as there was no loss of blood on admission, I determined to temporize a little, and abstain from any operative interference until the patient's strength was somewhat rallied, meantime prescribing absolute rest in the recumbent position, nourishing diet, and careful attention to the state of the digestive organs. As the os and cervix were particularly soft and yielding, I determined to administer the liquor secalis in ten minim doses, three or four times a day, in the hope of aiding the uterus to bring down the tumour within the range of the os. The treatment was steadily persevered in, and after she had been about two weeks in the hospital, very violent hemorrhage came on. The ergot was then given in larger doses, and with very good effect. I intended, when she got stronger, to use the uterotome as in the last case; but as the losses of blood were only occasional, and, with one exception, not very severe, I thought it better to wait until the repetition of the hemorrhage would justify me in interfering. The ergot was still continued, and caused a pretty constant sense of bearing-down pain on the uterus. There was also a good deal of watery discharge from the vagina, but no pus.

On January 3rd the patient presented a remarkably distressed appearance, and complained of a vastly increased pain. The swelling of the abdomen seemed to have gone down into the pelvis a good deal. I had her lifted on to the operation table, in order to make a more perfect examination. I was very much pleased to find that the tumour was protruding through the os uteri; the cervix was as tightly stretched over it as it is in the first stage of a severe labour over a child's head, and the woman had pains as severe as those then felt. After placing her in the lithotomy position, and getting Dr. Mackenzie to make firm pressure downwards over the pubes, I introduced a pair of very strong vulsellum forceps into the tumour, and made steady traction, giving regular intervals of rest, as if I were using the midwifery forceps. It required fully half an hour of patient perseverance in this manner to deliver the mass. It was quite the size of a cocoa-nut, and presented an appearance exactly similar to the white part of the flesh of preserved lobster. Professor Halford kindly examined a portion of it, and found the structure to be a mass of hypertrophied fibrous cells.

From this time the patient never presented an unfavourable symptom; she rapidly gained strength, her complexion cleared up, and she left the hospital quite cured on January 29th, 1866.

This was a most instructive case, occurring as it did in a woman forty-five years of age. The symptoms were, to a great extent, those that might be expected at the climacteric. The wretchedly sallow, unhealthy appearance, and the frequent hemorrhages, suggested the early stage of cancer of the uterus; indeed, so marked were these indications, that before I made a digital examination I almost certainly expected to feel a scirrhous os. The utility of the liq. secalis was well demonstrated, and I am sure that this medicine will be found more and more beneficial in such cases as this if fairly tried. I have proved its efficacy for many years also in cases of chronic uterine leucorrhœa. Had this case been left to nature, it would have brought the wretched patient to the state described in my former paper; sloughing of the mass would have reduced the system to the lowest ebb, and would, indeed, most probably have killed her, and the diagnosis would have been as in the case before alluded to, cancer uteri in the last stage. — *Australian Medical Journal, Feb.* 1866, *p.* 33.

71.—ON EXTREME SURGICAL TENDENCIES OF UTERINE PATHOLOGISTS; AND ON THE DIVISION OF THE CERVIX UTERI.

By Drs. E. J. TILT, HENRY BENNET, HEAD, GRAILY HEWITT, ROUTH, BARNES, and Mr. BAKER BROWN.

[The following discussion, at a meeting of the Obstetrical Society of London, embraces the opinions of some of our most eminent obstetric practitioners on modern surgical operations about the os and cervix uteri, and will be generally agreed with.]

Dr. TILT deprecated the extreme surgical tendency that seemed to characterise the present epoch. He gave as a proof of this tendency the frequency with which operations have been discussed at Medical Societies, the unnecessary multiplication of surgical instruments, and the warmth with which their invention or modification was supported. He likewise noticed two books which have lately appeared: one, a very important work by Dr. Marion Sims, in which constitutional means of curing diseases of women were almost completely ignored; and another, by Mr. I. B. Brown, in which it was recommended to cure hysteria, epilepsy, and insanity by amputation of the clitoris. Dr. Tilt stated that he had known the division of the cervix uteri to have been frequently performed or recommended in cases where he was able to pass the uterine sound, and he submitted that no practitioner was warranted in dividing the cervix either for sterility or dysmenorrhœa, when the cervical canal had that width; as microscopic animalculæ could find no difficulty in ascending where the uterine sound could pass. He alluded to the difficulty of passing a sound into the virgin womb, which did not interfere with the frequency of conception, in young women, soon after marriage. The author's experience led him to believe that the utility of dividing the cervix uteri had been unintentionally exaggerated. There were no statistics to show that conception was frequent after the operation; and he had frequently been consulted by those who had been operated upon during the last ten years, and who had remained barren. Dr. Tilt argued that there was so great a tendency on the part of the divided surfaces of the cervix to reunite, that the operation was generally useless unless followed up by dilatation; and he thought that in the majority of cases of uterine stricture dilatation was the safest and best way to relieve dysmenorrhœa and to facilitate conception. He wished the division of the cervix to be restricted to cases where the cervical canal was extremely narrow or the cervical walls very hard, and to cases wherein dilatation had proved a failure, or where there was flooding from uterine fibroids. He reserved his opinion respecting the value of the operation in cases of uterine displacement or of malformation; and deprecated the operation being resorted to as a kind of *pis aller* in those intractable forms of uterine disease in which relapses depend either on a congenital unhealthy tendency of the organs of generation, or on some deeply rooted constitutional taint. Dr. Tilt mentioned that in three of his patients the operation had been performed without the knowledge of the patients or their friends, and he took occasion to remark that this did not accord with the usually received notions of medical ethics.

Dr. HENRY BENNET thought that Dr. Tilt deserved the thanks of the society and of the profession for the paper read that evening. Although for the last seven years ill health had kept him out of active practice, he had continued to take the liveliest interest in uterine pathology, and had made himself acquainted with all that had been written or said on the subject. As a result he was deeply impressed with the idea that the therapeutics of uterine disease had taken of late too surgical a direction, and he thought, like Dr. Tilt, that this tendency required restraining, limiting, directing. After a seven years' absence from the debates of the society, he could not but feel that it was passing strange that he should have to rise as a conservative, and that in the very arena where he had many a time, in former days, defended progress, and where he had been opposed and stigmatized as a rash innovator. When he commenced practice in London twenty-three years ago, uterine therapeutics comprised little else but the treatment of cancer, tumours, prolapsus, and constitutional conditions. The most continued and irrational opposition met his efforts to establish more correct views—to demonstrate that physical means of investigation were as imperatively demanded in the study and treatment of diseases of the uterus as in those of diseases of the heart, lung, bladder, rectum, &c. By degrees, however, more reasonable ideas gained ground, and the senseless opposition to the progress of science was vanquished. Now it had entirely ceased, and had become a mere remembrance of the past. Indeed, as stated by Dr. Tilt, the danger rather appeared to be in going too far the other way, and interfering too much. This seemed probable when a recent surgical work on female diseases, written by a clever, experienced, laborious American surgeon, his friend, Dr. Marion Sims, proposed division of the cervix uteri on both sides, down to its vaginal attachments, as a remedy for all kinds of morbid conditions, for various deviations, and for sterility.

Indeed the Doctor, in one page, stated that he and his colleague in the Female Hospital at New York, performed this operation 500 times in two years ! Again, many recent writers and operators seemed imbued with the idea that the passage through the cervical canal to the cavity of the uterus ought to be, what might be termed metaphorically, as open "as a carriage door," constantly finding stricture therein, for which they operate by ruthless divisions, if it is not so. He (Dr. Bennet) believed that this view was founded in error, and that the greater part of these cutting operations were not in any respect called for or necessary. He believed also that this error would not be so constantly made were his discovery of a sphincter at the os internum recollected or recognised. This sphincter was a vital contraction of the circular fibres of the cervix at the os internum, similar in function to the sphincters which closed other cavities—the stomach, rectum, bladder. When the cold uterine sound reached it it contracted, and impeded its entrance into the uterine cavity, and a stricture was declared to exist. A wax bougie, No. 4 or 5, on the contrary, its extremity warmed by the hand, and slightly curved to the shape he had described as that of the uterine passages, generally entered with ease. The patent condition of the cervical passages which these authors appeared to consider necessary for conception was not natural, and certainly not necessary for the entrance of microsopic spermatozoa. It must not be forgotten either, in treating of sterility, that in England one married woman in six is sterile ; in America, according to Dr. Marion Sims, one in eight. The causes of sterility were very numerous, and were not to be removed merely by cutting a royal road for the spermatozoa. Moreover, these divisions of the cervix healed up, and in a few months the narrowed condition was as bad or worse than ever in most cases. Twenty years ago, at Sir James Simpson's instigation, he operated in many cases, and all but abandoned the operation on account of these relapses. Since then he had generally used very small sponges if he wished to dilate, and had never once had an accident. The attacks of inflammation that had occurred in the hands of others had no doubt been caused by the attempted dilatation of inflamed tissues. The cervical canal ought to be perfectly sound when it was interfered with. In conclusion he repeated that he quite agreed with Dr. Tilt that the uterus is now-a-days too frequently interfered with surgically, and that the indications for operations required better defining.

Mr. Baker Brown said he thought the paper was brought forward at a most appropriate time, for he perfectly agreed with the author and with the observation of Dr. Henry Bennet that operations upon the cervix uteri were performed too frequently, and without proper regard to preparatory and subsequent treatment. He was glad to have the opportunity of stating before the Society, in the strongest language, his reprehension of the rashness with which this operation was performed in both the out-patients' department of hospitals and the consulting rooms of the operator. He had always taught that the operation of dividing the os and cervix uteri was one of great danger; and although he had performed it a vast number of times, he had never done so without careful preparatory treatment, and the most absolute rest for two or three weeks after the operation. He thought the danger was also increased by the frequent division of the internal os. For his own part, in all cases of flexions, he simply divided the cervix up to, but not through, the internal os; but in all cases of uterine hemorrhage or intra-uterine fibroid tumours, he then carried his incision through the internal os. In all cases, immediately after operation, he plugged with oiled lint, and took every precaution to prevent the admission of atmospheric air. He believed the neglect of these precautions would generally account for the untoward results which so frequently followed the operation. He could confirm all that Dr. Bennet had said as to the opposition and persecution he had met with in reference to his treatment of uterine diseases; and when he reflected how triumphantly Dr. Bennet had overcome all his opponents by the truth of his practice, he (Mr. Brown) felt consoled for the opposition he received for publishing the results of his experience on a subject of which he as yet confessed himself to be but a learner. But as he had always, through a long professional career, immediately published any innovation which he had believed to be practically useful, so he would continue unto the end, feeling sure that the majority of the profession would always honestly investigate anything which he might place before them.

Dr. Head was of opinion that the expression "stricture of the os uteri" demanded a clearer pathological definition. He never had had the opportunity of seeing after death a stricture of any portion of the cervical canal, and thought the specimens must be extremely rare. He believed that cases of coarctation of the os uteri internum, not dependent upon organic disease or deviations of the uterus from its normal axis, may be attributable very frequently to spasm of the muscular wall of the uterus, especially at the orificial zone. Irritation acting upon the lining membrane of the uterus or cervix reflects itself to the muscular apparatus of the uterus much as occurs in cystitis. He concurred with Dr. Tilt in the belief that in numerous cases operative interference had been permature, and that we should hesitate before we appeal to the knife; indeed that it should be the last resource, and only employed after all constitutional and local measures had being found utterly inefficacious. He (Dr. Head) had lately contrived an apparatus, by means of which the vapour of chloroform unmixed with air can be passed into the cervical and uterine cavities, and had found chloroform vapour thus injected a remedy which was likely to afford considerable relief in cases of neuralgic dysmenorrhœa.

Dr. GRAILY HEWITT believed with Dr. Tilt that the operation of incision of the cervix uteri was too frequently, and therefore unnecessarily, practised. He differed from Dr. Tilt as to the indications for the operation. The uterine sound could frequently be introduced with a little patience in cases where the cervix was for menstruation purposes virtually strictured. He alluded particularly to cases, such as were by no means infrequent, where the canal was distorted and sinuous—a condition arising from thickening of the tissues of the cervix. The mucus membrane might be thickened also; but the chief condition was the irregular hypertrophy of the cervix itself. Then, again, he thought Dr. Tilt attributed too little importance to the effect of the presence of small fibroid tumours in determining flexions and consequent virtual stricture of the cervical canal. He believed many cases of cervical distortion and narrowing could only be dealt with effectually by means of a cutting operation; but the indiscriminate application of the operation was to be strongly deprecated.

Dr. ROUTH said the discussion proved how little the profession was agreed as to either the anatomy or pathology of stricture of the uterine canal. Anatomically, Dr. Savage did not admit a special sphincter at the internal os. Besides, did not circular fibres abound everywhere in the cervix? Then as to the seat of stricture, it was strange to find eminent men—accoucheurs—fixing stricture almost invariably at the external os; whilst others, quite as eminent, place it at the internal os. Then, again, was it to be said the uterine cervical cavity was never to be cut except in cases of stricture?—and could not the depth of the uterine incision be regulated according to the case? In chemosis of the eye, who could deny the advantage of relieving the congestion by incision? So in uterine disease a mucous lining might be so congested as to require scarification. The hysterotome effected this most satisfactorily. In parenchymatous uterine congestion itself it also was most beneficial. In conclusion, he could not allow physician-accoucheurs to remain under the stigma put upon them. Obstetric medicine was essentially *surgical;* and an accoucheur only proved his skill in acting *surgically* when prompt relief would follow, instead of acting *medicinally*, when cure would be thereby made very tardy, or not occur at all.

Dr. Barnes expressed his gratification at seeing again amongst them one who had rendered such eminent services to obstetric science as Dr. Bennet. Referring to his memoirs on Dysmenorrhœa and allied affections depending upon a peculiar conoid form of the cervix uteri with minute os externum, the President reminded Dr. Bennet that he had there quoted and adopted Dr. Bennet's views as to the os uteri internum. He believed that it was very rarely the seat of stricture, or the source of difficulties requiring division. All his experiences still pointed to the opinion expressed in that memoir, that the os externum was the seat of trouble. And when the peculiar formation which he had described existed, the consequences were very often severe, and even dangerous. He had seen retro-uterine hæmatocele caused by it, and had known examples of young girls dying in consequence. It was absurd to rely upon medicines, or indeed upon anything short of division of the os externum in such cases. As performed by him, the operation had always been safe. Speedy relief followed, and that in many cases which had undergone every other kind of treatment for years before. These were cases for surgical treatment.—*Lancet, Aug.* 25, 1866, *p.* 209.

72.—ON A NEW MODE OF TREATING EPITHELIAL CANCER OF THE CERVIX UTERI AND ITS CAVITY.

By Dr. C. H. F. Routh.

[The following article is an abstract of a paper read by Dr. Routh before the Obstetrical Society of London. The use of bromine was first suggested to Dr. Routh by his colleague, Dr. Wynn Williams. The two cases occurred at the Samaritan Hospital.]

In the first, the patient was thin, pale, and haggard, losing blood continually. There was a mass of fungoid epithelial growths, taking their origin from the os uteri, and about the size of an egg. The actual cautery was used to check the bleeding, and after the slough had come away a solution of bromine—five minims, to fifty of spirits of wine—was used. A piece of lint, the anterior surface of which was well saturated with the solution, was applied to the uterine diseased surface, and kept *in situ* by pledgets of lint. After forty-eight hours it was removed, and the part dressed at night with a poultice of lint dipped in warm water, and during the day warm douches were applied. In about a week a slough came away, and left a large healthy granulating surface. Tannin with glycerine was applied, and used daily. The patient also took internally the iodide of arsenic with extract of conium. After a period of ten weeks she was fat, hearty, and well coloured ; but as she occasionally lost a drop of blood, Dr. Routh carefully examined the internal surface of the uterus, and found about a quarter of its lining membrane affected with epithelioma. She left the hospital for some weeks, and on being readmitted a piece of wood about the size of the uterine cavity was prepared, and covered with cotton : the upper part was dipped in a saturated solution of carbonate of soda, the lower in the bromine solution, and it wa passed up and left within the uterus. Two or three further applications of bromine with glycerine were necessary, and the patient left the hospital with a movable healthy uterus.

In the second case there was a large carcinomatous mass, about the size of an orange, attached to the os, which appeared to be large cauliflower excrescences, breaking down readily and bleeding at the slightest touch. On the 20th January the mass was removed by the wire écraseur, and a few days afterwards the spirituous solution of bromine was applied. She took internally the iodide of arsenic and conium, and was treated in the same manner as the first case. She left the hospital on April 2nd, with a movable uterus covered with healthy mucous membrane, and looking herself fat and hearty.

The author remarked that he was quite aware that two cases afford an insufficient criterion as to the value of any remedy, and that time had not been allowed to prove that the cures were lasting. Notwithstanding these objections, he thought, at the same time, there were some considerations which made an early publication of these cases desirable. The author concluded by drawing attention to the care necessary in mixing the bromine with the spirits, which should be done very gradually, to avoid an explosion. He hoped others would try the agent he now brought forward, and give the results of their experience. He believed it to be a potent and useful remedy, and likely to prove of service, if not in the cure absolutely, at least in the arrest of the progress of cancer.

Dr. WYNN WILLIAMS said that he had applied solutions of bromine, in varying degrees of strength, in cancerous growths where there has been any breach of surface, for some nine or ten years ; and for the last two or three years to this disease when attacking the uterus, with the effect of destroying the cancerous mass, and causing its removal by sloughing. The first patient on whom he used it was a man suffering from epithelial cancer which had commenced in the lower lip, the soft parts having been almost entirely removed ; and wherever he was able to apply the solution of bromine the wound healed, until the whole external surface of it, extending, he might say, almost from ear to ear, had skinned over. The patient, however, ultimately died from extension of the disease to the neighbouring glands. Dr. Williams considered the beneficial effects of bromine were not confined to its corrosive or escharotic action only, but it acted also as a most powerful disinfectant, its good effects in this way being of very great service. He had seen patients with that peculiar cachectic, emaciated aspect so common in those suffering from open cancer rapidly improve in appearance soon after using bromine applications. He had found that in almost every case in which he had been able to apply bromine directly to the cancerous growth it had been followed by most beneficial results. He had frequently prescribed bromide of iron internally whilst applying the bromine externally, but thought its effect very problematical.

Dr. Rogers said he believed some of the previous speakers were labouring under an erroneous impression that the paper by Dr. Routh and the remarks of Dr. W. Williams tended to establish a new "specific for cancer." All that was desired to be made known was the fact that in some cases of epithelial cancer of the cervix uteri the bromine had proved a most energetic and valuable escharotic, destroying vascular growths, arresting hemorrhage and the prostration resulting from it, and checking all fetid and foul discharges. Healthy granulations followed its application, and the parts appeared free from disease. How long such improved state would continue could not at present be predicted. This was certain, that a most marked improvement took place locally and constitutionally. The patients would soon have died had not the disease been arrested; now they appeared restored to health and strength again. Of course, where the bromine could not be applied to the whole of the diseased parts, the mischief could not be arrested, and the disease proceeded on its fatal course. Bromine, like other powerful caustics, required great care and all the precautions mentioned by the author in its use. From its not being properly guarded, he (Dr. Rogers) had known mischief to arise which ought to have been prevented. He had used it himself, and had assisted the author with all his cases; and great credit was due to Dr. Routh for the skill, care, and perseverance exhibited by him.—*Lancet, Oct.* 27, 1866, *p.* 469.

73.—DIAGNOSIS OF HYSTERIA.

By Dr. C. B. Garrett, Hastings.

The morbid sensibility would appear to be pretty much (if not wholly) located in the skin. Take a case of excruciating pain under the left mamma. It is possibly put down as pleurisy, or, at least, pleurodynia. Say that there is a doubt. Well, then, either pressure or a sponge dipped in hot water applied to the spine about the lower dorsal vertebræ will cause an immediate and excessive aggravation of the original lateral pain; but pinching up the skin is an infallible diagnosis, for the pain becomes instantly intolerable. The same thing happens to the joints and any other parts where hysteria may develope itself; so also with those puffy tumours so often seen on the abdomen. I do not allude to the spine in these instances, but the pinching up the skin over the peccant part.

A curious case came under my care a few years ago, in which there was a lamentable failure in diagnosis. A young lady of the sort of temperament described by Mr. Skey took a long ride on horseback. On the following day her left elbow became painful and puffy. Leeches, &c., were had recourse to, and, as always happens in hysteria, depletion made matters worse. Blisters, mercury, and iodine followed in mournful succession. In about thirteen months after its first appearance she came under my care. The elbow was of great size, and the joint immovable; the case was clearly hysterical. Having administered the mist. ferri co. and tinct. valerianæ ammon. for a few days, I thought the time had arrived for attacking the anchylosis. My patient was placed erect in a chair; I supported the elbow with my right hand, and administered a smart blow or slap with my open left hand on her wrist. She screamed out that something had snapped, and freedom of motion was instantly restored to the elbow-joint. It would appear that in these cases there is a powerful antagonistic action set up by the flexors and extensors against each other, which is obstinately maintained till the balanced opposition is overpowered. Thus luxation of the lower jaw often takes place in hysterical subjects during gaping. I may here add that in the management of hysteria we must make it our motto to use kindness without sympathy, firmness without harshness.—*Medical Times and Gazette, Oct.* 27, 1866, *p*. 458.

ADDENDA.

74.—CONFESSIONS OF A LAUDANUM DRINKER.

By W. Whalley, Esq., Bradford, Yorkshire.

In obtaining the following brief and somewhat immethodical facts I experienced some little difficulty, owing to the reluctance evinced by the patient to reveal the true magnitude of her infirmity. For many years her demeanour was so circumspect that some of her most intimate acquaintances were not cognisant of the fact that she did imbibe laudanum. The subjoined particulars were fully corroborated by her husband, and their veracity is perfectly reliable.

On March 19th, 1866, I was consulted by Mrs. K., aged forty-four, who has had eight children, three of whom are living, their respective ages being eighteen, sixteen, and six years. She is about the average height, rather inclined to stoop, and moderately stout. Her hair is very grey, and her countenance had a rather withered, yellow aspect. Her eyes were glassy, and the conjunctivæ straw-coloured. When asked to put out her tongue she did so tremblingly, which was coated with a brown fur. She complained of headache, loss of sleep and appetite, intense thirst, and her bowels were slightly relaxed. She stated that owing to recent domestic adversities she had indulged in whisky as well as laudanum; of the latter, the the average quantity taken the last eighteen months had been a gallon a month. During this period the stomach has frequently retaliated and rejected its contents. She expressed a determination to relinquish the habit gradually if possible, believing that death would be inevitable if the accustomed stimulus was suddenly withdrawn.

About fourteen years ago she suffered from severe pain in the bowels, for which a friend advised her to take twenty drops of laudanum. This she found to afford the desired relief, and on each accession of pain at once resorted to the above remedy. In course of time the pain ceased to annoy her; but a desire for the laudanum had almost unconsciously been acquired, and unfortunately more courage and resolution were essential to its abandonment than fell to the lot of its victim. The habit became more and more confirmed, the craving for the stimulus became increased, and in order to produce the desired effect the dose was constantly augmented, so that after the lapse of a few years she was capable of taking a quart a week. More than this the stomach would not tolerate. Its effects upon her spirits were most exhilarating. She felt lively and cheerful, and could accomplish almost any amount of household work ; and instead (as is often the case) of suffering from constipated bowels, with accompanying headache, they were invariably relaxed, and there was entire freedom from headache. She slept well, her appetite was remarkably good ; in fact, there was very little, if any, disorder of the digestive organs. The organs of sense did not exhibit any signs or symptoms of impairment, except that of sight, which was not quite so good as formerly. If deprived of the laudanum for a single day, the most unpleasant symptoms came on ; loss of sleep and appetite, and an indescribable feeling of languor, with a corresponding depression of spirits, and slight involuntary motions of the limbs, all of which were readily relieved by the accustomed dose of laudanum.

When first consulted by my patient, I fostered the most sanguine hopes of being instrumental in enabling her to conquer her defect. The whisky was discontinued, and the doses of laudanum decreased, and by the aid of salines, &c., her distressing symptoms speedily vanished, her appetite and usual flow of spirits returned, and she was soon engaged in her customary domestic duties. At this period, however, my hopes were suddenly and unexpectedly doomed to disappointment from the absurd intelligence I received from her nearest relatives—namely, that it was their intention to allow her the laudanum, but in smaller doses, they being infatuated with the idea that to discontinue its use entirely would be to endanger her life.—*Lancet, July* **14, 1866,** *p.* **35.**

75.—PHYSIOLOGICAL ACTION OF NARCEINE.

In the last number of the *Journal de Chimie Médicale* there is an abstract of M. Linné's researches on the above subject, from which we perceive that the following conclusions have been arrived at :—(1.) Narceine is unquestionably of all the alkaloids of opium that which has the greatest narcotic power. *In the majority of cases* morphia and codeia do not produce as sound or as prolonged sleep as results from the use of narceine. (2.) Narceine differs from the other alkaloids of opium in producing little perspiration, and in causing no loss of appetite or nausea. (3.) So far from producing constipation of bowels, it causes relaxation, and, in large doses, actually gives rise to diarrhœa. (4.) It not only produces sleep, but diminishes pain. (5.) It has one peculiar action : it suppresses the flow of urine. For this reason M. Linné thinks it might be advantageously employed in cases of nocturnal incontinence of urine amongst children. But it seems to us that, until its action can be shown to be on the bladder rather than on the kidneys, its employment in such cases would be highly improper.—*Lancet, June* 9, 1866, *p.* 33.

76.—ON THE MEDICAL USES OF CHLOROFORM INHALATION.

By Dr. Charles Kidd, London.

The medical uses of chloroform, or rather its administration in medical as contrasted with surgical cases, begins to assume a form of very considerable interest and importance to the practising physician—not so much as an agent capable of removing slight or severe pain in neuralgic or purely spasmodic affections, as in controlling dangerous convulsions in the adult or infant, as a remedy also, or auxiliary of great usefulness in various forms of epilepsy, though not so effectual in simple hysteria, chorea, or delirium tremens. The medical uses of chloroform, in a word, have not been recognised sufficiently; its far wider and more brilliant aid to the operating surgeon having somewhat eclipsed its occasional applicability in the less demonstrative clinical wards of the physician. In cases of puerperal convulsions—which come under the notice of the general practitioner or young obstetric physician—the varied experience of all, or of the best obstetricians, agrees that whether we have albumen in the urine or not, the careful, skilful administration of chloroform, even before adoption of the lancet, seldom fails to afford marked relief, and that in a majority of cases we may dispense with the excessive venesections of former times; chloroform in such cases, of course, is not to supersede other means usually had recourse to, such as delivery, &c.

In cases of severe agony, and jaundice attending the passage of *gall-stones*, the inhalation of a drachm or two of chloroform will often act like a sudden charm in affording relief from pain and spasm. It is probable here, that there is produced a relaxation of the ductus choledicus, and associated muscular or nervous fibres of the adjacent diaphragm, duodenum, abdominal muscles, &c., all thrown into spasm by an irritating calculus; it is curious, too, that jaundice has been occasionally produced by chloroform, especially when given in small or irritant doses; but this is a much more rare phenomenon than the cure of jaundice by it.

A case of the following kind has come under notice:—A gentleman in the higher circles of society, an old gentleman subject to bad attacks of jaundice and gall-stone, in the country, was supposed to be at the point of death from "black jaundice" and impacted gall-stones; an entire week of horrible agony had passed; warm baths, blisters, purgatives, globulistic remedies, ipecacuanha, and every other conceivable remedy had been tried in vain. Opium had made things worse, and emetics no better; a fair trial, in one word, was given, or supposed to be given, to the good old classic abracadabra of Copeland and Watson, that

> "Old Experience which doth attain
> To something of prophetic strain."

Experience of all kinds had been summoned from the nearest country town; but with amiable suavity, Old Experience had pronounced the case incurable; it had been well excogitated that the bile secreted by the hepatic cells had probably regurgitated along the cystic duct into the gall-bladder; there stored up it became concentrated; how cholestrine (not Dr. Thudicum's blood corpuscles) had formed a calculus, with much more, all pointing to sadly established organic disease of the worst kind. In this emergency, as a matter of "ridiculous form," as it was said by Old Experience, a young but eminent city physician was summoned by telegraph—a man of the new school, eclectic and sensible, who are glad to accept remedies from whatever side of the compass they come.

"Have you tried chloroform," he ventured to ask in consultation? "Nothing like leather—of course not; but perhaps you would like to do so." To which he replied in the affirmative.

A few drops of chloroform, in fine, were tried, with the best possible result; the agony of the gall-stone yielded as if by some potent spell, the simple *rationale* of the cure being that it relaxed the muscular fibres, perhaps of the pylorus and duodenum, the gall-ducts, diaphragm, &c., and allowed the gall-stone to pass, whereas, previous purgatives, blisters, opium, &c., had only tended to derange the healthy sympathy which usually exists between the mucous membrane of the duodenum and the bile passages. In a sad case of this kind, not very long ago, it will be remembered, one of our most eminent members of Parliament died of the agony of gall-stones, and the derangement brought on by large doses of opium; it is probable one drachm of chloroform would have saved his life.

In some of the worst sufferings of uncomplicated asthma and in hooping-cough I have known the inhalation of chloroform to prove very beneficial; indeed it is clear that the often-vaunted popular cures of hooping-cough, by children inhaling the vapour at gas works, is easily and only to be explained by the calming influence on the glottis of some of the many gaseous carbo-hydrogens belonging to coal-gas, so like chloroform. I have known also the popular specific, or liniment of oil of amber, camphor, &c., for hooping-cough, to prove far more useful when chloroform nd ether were added to the formula—the liniment rubbed to the chest and neck, rather than in the time-honoured method, to the spine of the child. If hooping-cough (as it often is) happens to be mixed up with bronchitis, or if asthma be associated with emphysema, the chloroform will cause disappointment if it does not aggravate matters. A little antimony for the bronchitis may, however, be combined with the inhalation method, and will never disappoint in the bronchitis of children; the direct application of solution of nitrate of silver to the glottis, in hooping-cough, is also facilitated by the use of chloroform.

Again, in various forms of epilepsy, though theory at first contraindicated the administration of anæsthetics, subsequent experience has fully established the fact, that, contrary to presupposed views, chloroform in full doses controls the epileptic fit; and while good breathing is kept up there is no cause of apnœa, or alarm from suffocation. It is however as an auxiliary —in allowing a full examination as to the peripheral origin of the epilepsy, and surgical interference, by caustic or knife, leading to the cure of hundreds of such cases—that chloroform has remodelled the entire practice of epileptic diseases; the mechanism of the epileptic fit has now been so well explained by Brown-Séquard, Reynolds, and others, and the fit itself so often found to depend on external or peripheral sources; for instance, irritation of bad teeth, worms, external excitement of the genito-urinary organs, that a trial of chloroform, especially in female epileptics, seldom fails to yield suggestive results. Chloroform, on the other hand, is of less value (if not attended with positive risk) in hysteria and chorea; it is also of questionable safety in the debility of delirium tremens and puerperal mania; but in the other affections detailed—convulsions, asthma, hooping cough, gall-stones, obstetric practice generally, and many other purely medical cases—it will seldom fail to give most beneficial results.

In these observations I have not dwelt on the use of chloroform, except by inhalation. Every one, however, now knows the great usefulness of "chlorodyne" in all the several varieties of which Roscoe and others have shown that chloroform is the active agent, though at first denied.—*Dublin Quarterly Journal, Aug.* 1866, *p.* 83.

77.—SURGICAL USES OF LIQUID GLASS.

This is a silicate of potash, which is soluble in hot water, but insoluble in cold. It has been used for rendering structures incombustible, for protecting against moisture, &c., and the late Professor Schuh, of Vienna, employed it in surgery, and made a communication on the subject to the Medical Society of that city. He first used it in a case of arthritis of the elbow-joint. The solution is applied by means of a brush over the ordinary bandages. The advantages which this application is said to possess are the following. 1. It is simple, and easy of application. 2. It dries and hardens rapidly, requiring for this purpose only five or six hours. Desiccation may be favoured by using a solution evaporated to the consistence of a syrup. 3. It is very solid and impermeable. 4. It is readily removable by means of hot water. 5. It is very economical. Before putting on the bandages, a layer of liquid glass may be applied over the limb. At the Venice General Hospital, Dr. Minich has used the liquid glass with great advantage, in three cases of coxalgia, in seven cases of fracture, and in three cases of disease of the knee-joint. He has found it to possess superior advantages as regards solidity and durability. Plaster of Paris becomes sooner hardened, but is very heavy in comparison with the silicate of potash. —*British Medical Journal, May* 19, 1866, *p.* 525.

78.—CASES TREATED BY ACONITE INTERNALLY.

Under the care of Dr. Prosser James, at the City Dispensary.

Case 1.—A boy, æt. 13, seized with febrile symptoms on Monday; admitted the following Wednesday, complaining of sore throat and feverishness. Skin hot and dry; tonsils large and tender; whole fauces intensely congested; tongue "strawberry;" pulse full, 120; cough. Tinct. aconiti ♏vj.; aq. ℥iij. M. Sumat. ʒij. 3tia quâque horâ. On his next visit he was much better in every respect. The pulse had fallen to 98. One week after he commenced the treatment was discharged cured.

Case 2.—John B., æt. 35, after exposure to cold and wet, was seized with shivering, followed by heat of skin. On second day of illness was admitted with fever and sore throat. Voice reduced to a whisper; tenderness on touching the thyroid cartilage; fauces fiery red; tonsils somewhat swollen; with laryngoscope redness seen stretching down the larynx to vocal cords; epiglottis swollen and congested; pulse 110, full. Two drops of tincture of aconite (P. Lond.) to be mixed with a little powdered sugar and laid on the tongue every three hours. After eight doses complained of tingling all over. Congestion of throat nearly gone; voice better, but hoarse; skin cool and comfortable; pulse 80, small, compressible. Omit the medicine. The third day discharged cured.

Case 3.—Thomas ——, æt. 20, usual health good; states he has caught cold; on admission is very hoarse; has redness of fauces; tenderness over thyroid, and congestion of larynx on inspection by laryngoscope; skin hot and dry; thirst; pulse 95, full and hard. Three drops of the tincture mixed with sugar placed in his mouth. Numbness soon supervened; then a sensation of "pins-and-needles" all over him. About four or five hours later applied again, being frightened by these symptoms, besides a constant desire to swallow and fits of suffocative cough. On inspection the injection had gone, the velum lay loosely on the tongue, as though it were paralyzed—accounting for the desire to swallow and the cough. Pulse soft, 72. To take no medicine, but gargle the throat now and then with pure water. Next day all his symptoms had disappeared.

Case 4.—Henry ——, æt. 53, admitted November 1st, having been ill a week with pain in the side, cough, and expectoration. The pain is dull, and continued all over right side. Breathing very short and laboured; pulse full, 120; tongue furred; bowels open; expectoration "like white of egg," once a little rusty-coloured. On percussion and auscultation there is dulness and crepitus all over right back. Tinct. aconiti ♏xv.; aq. ℥xij. M. ℥j. 4ta quâque horâ.

Nov. 5th. Pulse much less full, only 86; all below the level of right nipple tubular breathing, with here and there points of crepitation. The whole inferior half of right back presents also tubular breathing and bronchophony. Cough troublesome; expectoration quite white. Continue the medicine.

8th. Pulse 80, soft and compressible; otherwise much the same; feels some numbness in his fingers. ℞. Pil. conii comp. gr. v. ter die sum.

12th. There is some increase of voice-sound over the right back, but no moist râles can now be heard; the numbness has not nearly gone from his two middle fingers; the cough is slight and easy; skin cool; no pain; all functions comfortably performed; was desirous of being discharged; was therefore discharged, with a few more pills, and directions to take care of himself.

Remarks.—These cases illustrate very well the practice of Dr. P. James in the internal use of aconite. He has employed it in several thousand cases of various diseases—mostly those in which there is increase of the heart's action. He employs it frequently as a febrifuge instead of salines, attributing to it the power of reducing the pulse and relieving the whole train of febrile symptoms. It is particularly useful as possessing certain anodyne qualities. There are few diseases in which the experiment has not been made. All cases characterized by nervous excitement seem to be benefited. Many forms of palpitation are at once cured. Dr. J. has frequently used it in organic cardiac diseases. In his practice aconite takes the place of digitalis as a controller of the circulation, in addition to the place of salines as febrifuges. The sensation of "pins-and-needles" shows that the drug is acting on the system, and on the appearance of this symptom it should be discontinued or the dose diminished. It is to aconite what salivation is to mercury, or muscular twitchings to strychnine. Its powerful action on the heart is shown by the rapid fall of the pulse in each of the cases reported above. Its local action on the fauces is well seen in Case 3, while in Cases 2, 3, and 4 the numbness was produced by a few doses.—*Medical Press and Circular, Aug.* 2, 1866, *p.* 121.

79.—ON THE TREATMENT OF ACUTE RHEUMATISM BY PERMANGANATE OF POTASH.

By Dr. James F. Duncan, Physician to the Adelaide Hospital.

[The following is an extract from a clinical lecture delivered at the Adelaide Hospital.]

In my last lecture I explained to you the views that I have been led to form of the nature of gout. I told you that I considered it depended upon imperfect oxidation of the albuminous constituents of the blood arising from impaired nervous energy. That these constituents, when about to be eliminated from the system underwent a metamorphosis, first into lithic acid, and subsequently into urea by combination with oxygen in different proportions, and that when the process was arrested in its course, lithic acid accumulated in the circulation in the form of lithate of soda, and gave rise to the peculiar symptoms which constitute the gouty paroxysm. I further mentioned the connexion which I believe exists between this imperfect oxidation of the nitrogenous compounds and impaired assimilation in the digestive system—a connexion which has induced me to resort to the use of hydrochloric acid in the treatment of the disease, and as you may have yourselves observed, on several occasions with very decided benefit.

I proceed to-day to speak of the kindred affection, acute rheumatism, which presents many points of resemblance to gout, but which appears entirely different in its nature, to affect a different class of persons, and to require a different plan of treatment. This disease, as well as the former, seems to depend upon imperfect oxidation of the blood, but the particular constituents which are implicated in the two diseases are perfectly distinct. In gout it is the nitrogenous compounds, in acute rheumatism it is the non-nitrogenous or starchy. These latter seem naturally to pass through the several stages of conversion into sugar, lactic acid, and carbonic acid—changes which depend entirely upon the increasing amount of oxygen which enters into combination with the original hydrocarbon. When the last stage, that of carbonic acid, has been reached, the transmuted starch having fulfilled its office in the economy passes off from the lungs in the act of respiration. I need scarcely remind you that while the nitrogenous articles of food are supposed to contribute to the support of the animal tissues, the carboniferous are believed to be principally engaged in maintaining combustion. It needs no argument to prove that if the series of changes I have detailed do not proceed through their entire course, the imperfectly oxidized products, whether in the form of sugar or of lactic acid, must accumulate in the blood and give rise to disease, constituting in one case diabetes, and in the other rheumatic fever. That lactic acid, when in excess, is capable of producing all the phenomena of acute rheumatism, including the endocardial deposits on the valves of the heart, has been shown by Dr. Richardson of London, so that no reasonable doubt can remain as to the efficient and proximate cause of the affection; and as I have attempted to show that the imperfect oxidation of the nitrogenous compounds in gout is due to impaired nervous energy, which fails from this circumstance to carry out to completion the chemical changes that take place in health, there is reason to believe that the imperfect oxidation that occurs in acute rheumatism is due to a similar cause, but arising under different circumstances and in a different manner. Rheumatism can generally be traced to an attack of cold disturbing the functions of the body generally, and producing well-marked fever. It would not be difficult to show, from the way in which the various functions are affected, the arrest of the secretions, the change in the temperature of the body, &c., how deeply the nervous system is involved in this state and how much its energy must be depressed.

The view just presented of these two diseases appears to me to be both interesting and important, and sufficient to account for many of the differences which are known to exist between them. For example, while the one prevails chiefly amongst the upper class of society, because of the abundance of the nitrogenous compounds contained in animal food of which they partake largely, the other is common among the poor, whose diet consists, to a great extent, of vegetable matter, and while gout is a disease of mature life and even of old age, rheumatism occurs at all ages, not excepting the very young, because all are equally liable to suffer from cold and wet, whereas a long course of indulgence of the appetite seems generally necessary to pave the way for the development of gout.

It may perhaps seem strange that if the views here propounded have any foundation in fact, that the matter has not ere this been made the subject of scientific experiment and reduced to demonstration. But the fact is, that with all our boasted progress of late years, the attempt to penetrate the mysteries of nature is not so easily to be accomplished. Every one acquainted with organic chemistry must be aware of the extreme difficulty of determining the precise constitution of complex substances of this class as compared with those of an inorganic nature. Many different organic compounds have yet the same molecular constitution so far as the number of their constituent atoms is concerned, yet owing to differences in the manner in which these atoms are arranged among themselves, their peculiar properties are totally unlike; and different analysts arranging the constituent elements of these substances according to their individual views will give a different account of their respective constitutions. This difficulty becomes still more remarkable when the subject of our inquiry is a living body and embraces the consideration of vital energy present in the body to be examined and influencing the result. You are all, I dare say, familiar with Claude Bernard's investigations regarding the liver. He first thought that the liver possessed a power of producing sugar out of the blood in its progress to the lungs, and that the sugar thus formed was burnt off in the act of respiration, so that no trace of it could be found in the left ventricle of the heart. He subsequently modified this opinion, so far as to believe that there was in the liver itself a peculiar substance which he named glucogene which had the effect of changing by contact the starchy elements of the blood into dextrine or grape sugar; but the late researches of Dr. Pavy, repeated and confirmed by Dr. Robert Macdonnell of this city, have shown incontestably that what Bernard considered was the natural and normal condition of the liver, was really a post-mortem effect occurring almost at the instant of death, and in all probability the result of the cessation of the energy of life. I mention this to show not only the extreme difficulty of determining questions of this kind, but the strong colour of probability that it gives to the hypothesis which I have here propounded, for if the formation if dextrine be kept in check by the vital force so long as that force is maintained in its full vigour, and if, as appears by these experiments, sugar may be detected in notable quantity in a remarkably short space of time after an animal has been killed, so short in fact as merely to allow of preparations being made for carrying on the investigation it is extremely probable that anything which materially weakens the nervous energy must favour the production of this very substance which is the preliminary step to the formation of lactic acid, and which unless carried forward to the further development of carbonic acid, must lead to acute rheumatism.

Not that you are to suppose that the correctness of these views is dependent in any sense upon the sugar-producing power of the liver being sustained by the further light which physiology may throw upon this subject. The remarkable point of his investigations was, his showing that the liver could form sugar out of nitrogenous substances—materials naturally unsuitable for the purpose. But I need scarcely tell you that the more obvious sources of sugar—the articles containing starch which are used as food—find their way into the circulation without passing through the vena portæ at all, and consequently without traversing the liver. In whatever way, however, they may be introduced into the blood, they seem to have this useful purpose to serve in the economy, to support the changes occurring in respiration, and to require to be formed into carbonic acid in order to be eliminated from the system.

Let me now very briefly attempt to apply this view of the nature of acute rheumatism to the question of its proper treatment. Setting aside what may be regarded as the treatment of collateral symptoms by special remedies, such as the abatement of pain by the use of narcotics, the plans in ordinary use for the cure of the disorder may be classed under the heads of the antiphlogistic, including bleeding, purgatives, and mercury; the tonic, including the administration of bark, nourishment, and stimulants; the alkaline, including the alkaline carbonates, inténded to neutralize the lactic acid; and lastly, agents which contain oxygen in large quantity, and which may be supposed to part with this principle to promote the complete transformation of the lactic acid into carbonic acid. Each of these plans of treatment has its advocates, and no doubt peculiar circumstances in individual cases may render one plan suitable to one case and another to a different one. I am by no means disposed to recommend a routine treatment to any form of disease; on the contrary, I would myself adopt the eclectic plan, and strenuously advise you to do the same. My purpose at present is only to speak very briefly of the last of these plans, which embraces the administration of nitre, of citric acid, and of the remedy you have seen me using of late—the permanganate of potash—a remedy which I am not aware has ever been tried in this disease before, but which I was induced to resort to from a consideration of its chemical constitution. All these substances contain oxygen in large quantity, but the peculiarity of the permanganate is this, that it holds this element in a very loose affinity; so loose, in fact, that it is almost impossible to combine it advantageously with any other substance, as it undergoes decomposition and becomes practically inert. The effects we have seen it produce for so far are these: it cleans the tongue, relieves pain, acts slightly on the bowels, and removes turbidity and fætor from the urine where they have existed. The only drawback to its use has been that several patients have complained of its unpleasant taste. The form in which it has been given has been that of solution of Condy's disinfectant fluid in the proportion of one part to seven of distilled water. Half an ounce of this mixture every second hour for a dose. The strength of this preparation, as given by Neligan, is 9·26 grains to the ounce, while the ordinary solution of the Pharmacopœia is only four grains to the ounce. In classing nitrate of potash among the oxidizing agents, I know that many persons will object to the view thus suggested of its *modus operandi* and maintain that its beneficial effects are solely due to its property of increasing the secretions from the skin and kidneys; but I think it is not unreasonable to suppose that among the various chemical changes that take place during its action, some portion of it may be decomposed, to answer the purpose in question. Nitrate of potash, however useful in some cases, from its depressing influence on the circulation, is not suited in my judgment to any instances when the vitality is not vigorous. Lemon-juice is, I need scarcely tell you, both palatable and safe, but is occasionally apt to act on the bowels, as happened in one case, for which reason it had to be laid aside after having given temporary relief.—*Medical Press and Circular, May* 16, 1866, *p.* 501.

80.—DENTAL INSULATOR FOR ANÆSTHETIC OPERATIONS.

By Francis McLean, Esq., Dental Surgeon to the City of Dublin Hospital, &c.

Some months since when the anæsthetic spray producer was on its trial, it was reported to be most successful in all minor surgical operations, excepting the extraction of teeth, in which most painful one its efficiency was only doubtful. I then laid before the readers of the "Medical Press and Circular," in its issue for March 14, 1866, the result of my experiments with it in dental operations, and I arrived at the conclusion "that the anæsthetic effect is complete whenever the spray can be properly applied." In its application I found a number of difficulties to contend with, but the greatest was to prevent the tongue and cheek from interfering with the ether spray when applying it to the teeth in the lower jaw. With a view to obviate this difficulty, I have contrived an instrument which can be easily used without the assistance of a third party—which will expose an uninterrupted view of the tooth to be extracted, and of the parts necessary for the spray to play on—very simple in its construction, and can be held in the mouth lightly by one hand of the operator, while his other is engaged directing the ether jet.

The instrument, manufactured of silver, consists of two valves, curved so as to give a sufficient space round the teeth to which they may be applied for the introduction of the double jet, and hinged to each other, to allow them to adapt themselves closely to the lower part of the jaw. To the inside is attached a plate for depressing the tongue, which it effects without controlling its movements or causing the unpleasant sense of sickening which the use of the spatula is subject to. To this depressing plate is attached the handle movable on a pivot, so as to allow of its being drawn to one side. When the instrument is placed *in situ*, it perfectly protects the tongue and cheek from the ether, the two valves being pressed together by the parts, fit closely to the gum covering the lower part of the alveolar process, while the tooth and its surrounding parts are seen in the centre perfectly insulated.

The patient keeping the mouth well open so as to allow the ether to evaporate quickly, can at the same time work the hand-ball bellows as the operator may direct. The moment the parts become blanched, the surgeon dexterously withdraws all, and without delay extracts the tooth, which I have no hesitation in saying can be done painlessly in a large majority of cases, *provided you use anhydrous ether with Richardson's double spray producer in proper working order.* Too much attention cannot be given to these requirements, for in cases in which I have seen the apparatus fail, and so fall into disrepute, the causes have been its not working properly, and the impurity of the ether used. With regard to the objection of the ether entering the throat and tickling the fauces for the few seconds which the application requires, this can be avoided by protecting it with a napkin, and requiring the patient to breathe through the nose.

This instrument, to which I have given the name Dental Insulator, I place before the profession as the result of my exertions to facilitate the use of local anæsthesia in dental operations, not doing so, however, without fully testing its efficacy. Having done so, I feel satisfied that by its aid we shall have more success in operating on the teeth of the lower jaw than has hitherto been experienced. — *Medical Press and Circular, July* 11, 1866, *p.* 34.

81.—TOOTH CEMENT (STEHLE.)

Gutta percha, 5 parts; white wax, 1 part; oil of cloves, a few drops. (*Wittstein's Vierteljahresschrift f. Pharmacie* p. 2, xiv.) Another (Sorel):—A light oxide of zinc is prepared by moistening the ordinary oxide with nitric acid, and then igniting it. Thus prepared, it is made into a soft paste with a solution of chloride of zinc, having a specific gravity 1·9 or 2·0. This soft mass speedily acquires great hardness, which it permanently preserves. If a grey colour is required, the least trace of carbon may be used, got by holding the pestle with which the paste is made over the gas for a moment. A trace of sulphide of cadmium will produce a yellow tint.—*Year Book of Pharmacy.—Medical Press and Circular, May* 9, 1866, *p.* 494.

82.—ON ELECTRO-THERAPEUTICS.

By HARRY LOBB, Esq.

Static Electricity. —Static electricity is a force manifesting itself through the agency of matter by attractions and repulsions, and these are its most important conditions in the inorganic kingdom; but when we come to study it in vitalized bodies. we find that it undergoes a new series of changes, differing in their characteristics from what we have been led to anticipate by its phenomena in the inorganic kingdom.

I must, however, before passing on to the more important field of dynamic electricity, give you some idea of static electricity; but I shall not detain you long on this subject.

The theory, explaining the various electrical phenomena which is now most in favour, is that of Symner. He assumes that all matter has associated with it a certain quantity of a subtle imponderable fluid, made up of two separate conditions of the same fluid, so that in combination they neutralize one another; but when by the action of some force, they become separated, the one is termed positive or vitreous electricity, the other negative or resinous. This is a pure hypothesis, and really means nothing, but affords a simple method of explaining many electrical phenomena.

Now we have the means of detecting the presence of one or other of these electrical "fluids" by an instrument termed an electroscope; here is one termed *Volta's condensing electroscope.* You will perceive that there are two pieces of gold-leaf hanging from the lower brass plate, the electricity is in a neutral condition; and the gold leaves are consequently at rest; but we have the power of detecting very small quantities of free electricity with this apparatus. Suppose we wish to detect the presence of free negative electricity in a piece of sealing-wax; we place it after having rubbed it, in contact with the lower or collecting plate.

The upper or condensing plate is connected with the earth, by touching with the finger, and the plate is electrified positively, in contradistinction to the lower plate, which is electrified negatively by means of the wax; but the leaves do not sensibly diverge, as the opposite electrical fluids attract one another, and thus become located almost exclusively on the two plates.

If the upper plate be now removed, and with it the source of the positive electricity the negative electricity freely passes to the leaves; and being both negatively electrified, they repel one another and fly apart. Now, after a time, the leaves fall together again from the negative electricity on the leaves combining with the positive electricity in the air: if the air is very dry, this takes place slowly; if moist, rapidly.

This will give you some idea of static electricity, in reference to its combined or neutral state, and its decomposed or active condition.

The states of attraction and repulsion are the most important features of static electricity; but only secondary to these is the inductive state, which electricity has the power of setting up in other bodies at a distance. Our great Faraday considers this power of inducing an opposite state of electricity in matter in a neutral state by the action of an electrified body in its neighbourhood, to take place through the surrounding medium, which must be, to a certain degree, non-conducting; thus, in air, the atom in juxta-position to the electrified body by the inductive power of that body is polarized, the contrary electrical state is induced in the adjacent portion, and the like state is induced in the further portion of the atom; and in like manner from atom to atom, until the neighbouring body is reached, which is consequently in an opposite electrical condition to the inducing body. Now this induced state can only take place if the bodies are at a certain distance, and the quantity of electricity in the electrified bodies of a certain amount; for upon the bodies being made to approach each other, coming within a certain distance so that the attraction of the two electrical conditions overcomes the resistance of the medium, the electricities fly together, and equilibrium is established; or again, if the quantity of electricity in the electrified body is increased, it will at length overcome the resistance of the medium, and again equilibrium will be re-established by combination.

This leads us naturally to the subject of *tension*, which may be defined as the desire of the accumulated electricity to combine, by overcoming the obstacles offered by the medium.

Static electricity is chiefly induced by friction; it accumulates on the surface of bodies, and manifests its presence by attractions and repulsions.

And here I shall dismiss static electricity, for we shall have very little further necessity to touch upon this agent, as I shall be able to prove to you that we have a much more valuable agent in dynamic electricity for the treatment of disease.—*Medical Press and Circular, June* 13, 1866, *p.* 619.

83.—THE MUTUALLY ANTIDOTAL PROPERTIES OF OPIUM AND BELLADONNA.

By Dr. Henry S. Downs, of New York.

This is the subject of a paper by Dr. Downs, published in the Transactions of the State Society of last year. *Eleven* cases are detailed, of which we give a synopsis.

Case 1.—Infant, three and a half months old, had been given about one teaspoonful and a half of a preparation of paregoric nearly one-half stronger than the tinct. opii camph., U.S.P. Narcotism, deglutition difficult, pupils contracted to the size of a small pin's head. Child was only kept aroused from stupor by severe and continued agitation. About six hours after the administration of the dose, ten drops of tinct. belladonna were given, and repeated at intervals of fifteen minutes until forty drops were given, when the pupils were dilated to double, and in one hour to treble the former size. Improvement rapidly followed. (Dr. Downs)

Case 2.—Woman, æt. 35, had taken one-half ounce of laudanum suicidally. Seen an hour later, partially sensible. *No* emesis could be induced by ipecacuanha, sulphate of zinc, tartar emetic, or mustard. She became insensible, pupils contracted to the size of a pin's point; profound stupor, from which she could not be roused. One drachm of tinct. of belladonna was then given, followed by two grains of the best English extract of belladonna every twenty minutes, until six grains were taken. Soon after the third dose the patient vomited, the ejecta smelling strongly of opium. Sensibility gradually returned, and at the end of eight hours pupils were natural, sight restored, and consciousness complete. (Mr. Loines and Dr. Jones.)

Case 3.—Woman, had taken one and a half ounces of laudanum. All the symptoms of opium poisoning present. Emetic first, then a reliable tincture of belladonna, thirty drops every half hour, until ninety drops were taken. Rapid recovery and no indications of her having taken belladonna. (Dr. Wm. H. Thompson.)

Case 4.—Soldier, took two ounces of laudanum and smoked one grain of opium. Three hours afterwards was found comatose, pupils contracted, and slightly spasmodic. Forty drops of tinct. belladonna were given at half-hour intervals, until he had taken 120 drops, and then in half an hour twenty drops more, when he began to improve, and entirely recovered in a few hours, with no signs of having taken belladonna remaining. (Same authority.)

Case 5.—Soldier, took over an ounce of laudanum. About an hour afterwards drowsy, face flushed, pulse low, pupils much contracted, hands twitching, tendency to convulsions. One grain of extract of belladonna every half hour until three grains had been given. Two hours after first dose of belladonna, the pupils had dilated, pulse and skin natural, and able shortly after to go on duty. (Dr. Charles D. Hackley.)

Case 6.—Infant, eight weeks old, had been fed on Mrs. Winslow's soothing syrup until complete recovery was impossible, even by the aid of belladonna. (Dr. C. C. Terry.)

Case 7.—Belladonna poisoning occurred in a young woman who used belladonna pessaries for the purpose of allaying the excruciating pains of ovarian dysmenorrhœa. The toxical effects were suddenly developed to delirium. One and a half grains of Magendie's sol. sulph. morphia entirely removed the symptoms of poisoning an hour after delirium appeared. (Same authority.)

Case 8.—Belladonna poisoning from use of belladonna pessaries. Opium used with same good result. (Same authority.)

Case 9.—Child, through mistake, took one teaspoonful of laudanum. An hour later comatose and incapable of being roused. Ten drops of tinct. belladonna every hour until sixty drops were taken, when narcotism subsided and the patient slowly but gradually recovered. (Dr. J. P. Garrish.)

Case 10.—Man, took one ounce of laudanum. When seen, two hours later, feeble pulse, contracted pupils, stertorous breathing, and could not be aroused. Emetic first, and twenty drops tinct. belladonna every hour until about ninety drops were taken, when he began to show signs of consciousness, and by the aid of coffee, stimulants, and the cold douche, was gradually restored. (Dr. Garrish.)

Case 11.—Woman, took ten grains of opium. In one hour was found in semi-comatose state, pupils contracted. Emetic, then twenty drops of tinct. belladonna every hour until sixty drops were taken, when consciousness began to return, and she recovered.—*Philadelphia Medical Reporter.—Medical Press and Circular, April* 11, 1866, *p.* 369.

84.—CHLOROFORM IN DYING.

By Dr. JOSEPH BULLER, Physician to the Royal South Hants Infirmary.

So many cases where most attention is expected, are those where disease can only be palliated, that palliation becomes a very important part of our duties. Few of this large class are more distressing, than when extreme restlessness and sleeplessness accompany the exhaustion of the last days or weeks of the life of the very aged, especially when (as is often the case) the mental consciousness is still active, and the failure of power in the vital organs is actually felt, with none of that physical courage to bear the suffering, which the same patients had when younger and stronger. That true health, or at least that tenacity of life on which its long duration depended, keeps them alive and suffering, and conscious of this suffering, for a length of time tedious to themselves and often most wearisome to those who watch over them, and who look (so often in vain) to medical aid for an alleviation which would relieve themselves as well as the patient.

Small opiates, which at an earlier stage may have been useful, at this later one often aggravate the distress instead of soothing it ; and it is in this condition that the cautious inhalation of chloroform is a great boon.

A lady, aged 82, had been for some years confined to her bed and sofa, and, without any appreciable bodily disease, was now gradually sinking. For some years she had suffered mentally from depression taking a religious form as to her soul's safety, and so persistently, that it seemed like a delusion, from the powers of the brain becoming enfeebled. But she had a good appetite ; could employ herself much with finger-work ; and, though confined to her room and much in bed, had a very fair amount of bodily health. Some months before her death, the delusions were attended with more excitement and irritability ; she slept less, and often not at all ; and her appetite gradually declined. Small doses of liquor opii sedativus (from five to ten drops) at night soothed her at first ; but, as her strength diminished, the same doses excited her brain, and she lay in a very distressing state of restlessness and prostration, exacting constant attention from those watching her. She lost her appetite and power for solid food, and could only take small quantities of beef-tea and weak brandy and water. In this painful condition, about five weeks before her death, I recommended the inhalation of twenty drops of chloroform at a time at bedtime, on a handkerchief ; having first given it to her myself, to see whether it soothed and was agreeable to her, or otherwise ; and it was found that this prevented the exciting effects of the liquor opii sedativus until its narcotism exhibited itself, and by both together she had quieter nights. But, after a few days, the opiate was discontinued, as it excited, and chloroform alone used, with so pleasant a result to the patient herself, that she frequently, by a sign, indicated her wish to inhale it, and was partially for a month and wholly for five days before death kept almost constantly under its moderate influence. The effect was to quiet the delusions, to make her mind peaceful and happy, and also to raise the pulse and respiration. Her daughter observed this, and I found it to be the case. When she was so weak that the pulse could only be felt with difficulty, a short inhalation of chloroform rendered it distinctly perceptible, and the respiration became slower and more natural.

At my request, one of this patient's daughters stated in writing their observations on chloroform in their mother's case, as the inhalation was carried out by these ladies and a middle-aged sensible attendant.

"A pleasing feature in her case," writes the daughter, "was that chloroform never made her really insensible; it only lulled her pain, gently calmed her spirits, and frequently, but not always, sent her to sleep for a few minutes, when she would awake quite herself, with a perfectly natural look and manner, and perhaps ask how long she had been asleep; and this after so many months of fearful excitement. We felt nervously anxious at first of giving her too much, and we never omitted to watch the pulse. One morning, after breakfast—a time when she was generally low—I had given her the usual dose of brandy and water, when she said, 'I do not like the brandy as I did; give me some of the nice stuff to smell' (meaning chloroform). I did so with some anxiety, as she was so low, when, to my great relief, the pulse gradually rose. I continued giving small doses at short intervals, when it very soon regained its usual strength. After this, we constantly noticed the same thing; and we no longer hesitated to give her as much as she craved, especially as the breathing powers were much relieved and became more free, and the countenance took a peaceful and happy expression, such as we had rarely seen in her of late years. To our minds, this craving was an instinct of nature. She had for a long time been asking for something to smell, and nothing we could get for her seemed what she wanted; but, when she had once felt the effects of the chloroform, she never asked for anything else, but for that constantly.

"My mother used just under a quart. The greater part was given during the last week or ten days of her life; but, the night before she died, she inhaled nearly half a pint. Still she was conscious till within six or eight hours of her death; at that time, we believe she became quite insensible."

Statements of this sort by intelligent friends, who watch effects and describe them without any bias, are especially valuable.

This statement is satisfactory as to chloroform soothing the mind; for, under its mild influence, the patient was more like herself in the most tranquil periods of her life, and altogether different from that excited and restless or depressed condition which, as her bodily powers failed, added so much to her sufferings. It is also valuable in showing that, in certain weak states of body, chloroform strengthens the pulse and respiration—a fact often observed by those who watch the pulses of patients undergoing operations under chloroform. If the dying state be reckoned by the respiration becoming quicker and the pulse failing, this patient took five days in dying; and, as the chloroform so obviously increased the strength of the pulse and breathing, it rather prolonged than shortened life. Indeed, it seemed to act like the tonic stimulus of food or wine in a stronger bodily condition, and at a time when neither of these could be taken.

This was one of the few cases in which (no *post-mortem* examination having been made) the only return of the cause of death I could make was the decay of old age. The lungs, heart, digestive organs, and kidneys, were without disease; and the delusions were attended with no paralysis or loss of cerebral or nerve power.

In the next case, although the patient was 75 years of age, and had been breaking for two years, he did not die of old age, but was greatly worn down by pain. He was well known in this county as a sportsman. He had hunted, shot, and fished from his youth, and still shot all the season; indeed, his last illness was brought on by exposure to cold in shooting, beginning by severe catarrh, and followed by violent pain of the left side of his face and scalp and ear. The pain resisted quinine and other neuralgic remedies, and its obstinacy was eventually explained. He had been deaf for some time; but increased deafness of the ear, with tenderness and some fulness over the mastoid process of the left temporal bone, indicated disease of the petrous portion. He was a man who had been able to bear pain unflinchingly. He was organised for a sportsman—tall, lean, muscular, no fat, large-chested, and bony. In hunting, he had at various times broken or dislocated several of his long bones; and his brother told me he had seen seven men with pulleys trying to reduce his dislocated shoulder, whilst he uttered no complaint; and the surgeons said (in that heroic age of remedies) they could not make him faint. But this pain was so excruciating that it wore him down. It was always there, but at times in acute paroxysms. He had chronic cough and expectoration from bronchitis, and latterly passed much pus from the bladder, giving suspicion of pyæmia, with complete loss of appetite and failing strength. He could never in his whole life take opium, as it excited him; and when he had it now, it produced no relief, but distressing sensations and sickness. In this condition, and about three weeks before his death, when all hopes of his recovery had passed, I recommended him to inhale chloroform. It relieved the pain, and gave him bodily comfort. The effect, he said, was like champagne, when he could drink a bottle of it. As he became weaker, he increased the quantity of it, and kept himself much under its influence. In the last five days, sixty-three ounces of chloroform were used. He was a strong-willed man, who would do as he liked; and, having once felt the agreeable relief which choroform gave him, he compelled his niece (who watched him) and his servant to wet his handkerchief with it as often as he called for it. It rendered his last days bearable, and indeed comfortable, instead of a period of excruciating pain. There was no *post-mortem* opportunity of seeing the disease which produced this great suffering; but, as the mastoid process was tender on pressure and swollen, and the skin somewhat red, with complete deafness, it was evidently from diseased bone. It recalled a case I saw in February, 1863, of a man who seemed literally to die of pain referred to the sacrum and coccyx, which came on after exposure to wet three months before admission into the South Hants Infirmary; and by no remedies could the pain be removed. Subcutaneous injections of morphine, chloroform, veratrine, aconitine; externally, blisters, with morphia, hot hip-baths, were thoroughly tried; externally and internally, opium, chlorodyne, cannabis indica, quinine, iodide of potash, and guaiacum. Short relief was given by relays of four leeches, but this only for a short time; and the poor fellow was actually worn down, and died from this pain. My friend, Professor Aitken, of Netley, kindly examined minutely a portion of the sacrum and coccyx I sent him, and the cause was discovered to be vascular tumours in the bony structure. He thus described them. "In both pieces of bone, after the flesh had been removed, vascular tumours are seen. One of them is nodulated, and of the circumference of a shilling; the other less. They are embedded in the spongy bone, and one at least presses upon the nerves as they make their exit through the sacral foramina. I am of opinion that more of these little tumours have existed in the substance of the sacral bones; perhaps also in the spongy parts of the vertebræ. They seem to me to be of a varicose nature, and connected with the venous system, rather than the arterial."

This case, as in the former, came on after exposure to cold. He had lived very freely, occasionally drinking very hard; and, though only sixty-two, was a worn-out man. He had Bright's kidneys.

It did not occur to me to make him inhale chloroform, as I should do now; for it was one of the most distressing diseases I ever watched, as nothing alleviated his constant pain, and (as the cause showed) nothing but such an anæsthetic as inhaled chloroform would have destroyed pain from tumours pressing on sensitive nerves amongst bone.

The only other case in which I have given chloroform to the dying was that of a lady, many years ago, who was subject to very painful attacks of gall-stones. Chloroform inhaled was the only relief. After several gall-stones had passed, at varying intervals of weeks and months, one attack came in which the pain never ceased, and she died after many days, deeply jaundiced. Her son, who was studying medicine, administered the chloroform. "It had the effect," he writes, "of very quickly lulling the pain; and, the moment she was out of pain, I desisted. At such a moment, she was sometimes conscious of my presence, sometimes not. The amount of chloroform used altogether was very large indeed." I found, *post-mortem*, a gall-stone of the size of a small marble, impacted in the common duct, two inches from the duodenum. The liver was very large and light yellow; and there was an ovarian tumour, with hair and bones in it, the pressure of which on the crural nerves during life had caused much suffering. She had been an invalid for a long time. In this case, also, when the patient had felt the relief from chloroform, she would have it given to her. I have seen one patient die from a gall-stone rupturing the duct; and the agony is such, that any amount of chloroform which gave ease would be justifiable; so would it be in cases of rupture of the stomach and intestines. But, in these cases, it must be given as long as life exists; and, when once its relief is experienced, we may be sure the patient will insist on its continuance to the fatal end.

I look back many years with regret to a night during the whole of which a lady of the most sensitive nervous organisation died in inconceivable suffering. Dr. Baillie said to her in her girlhood, "You will have a great deal to suffer in your life, my dear, but don't talk about it;" and his prescience was too true. For many years before her death, she constantly suffered abdominal pain, restlessness, and general distress; and latterly the cause of this was explained by the discovery of an abdominal tumour. This suddenly burst internally; and, after twenty-four hours of what was literally horrible torment, she died. Having watched the case for years, I knew she must die; but, from the weakness of her heart, I feared chloroform might extinguish life at once. With my present experience in chloroform, I should have given it freely, with the belief that the chances would be in favour of its rather prolonging life, which was shortened by the pain; and opium gave no relief.

The rule, in advising chloroform in these cases, is to judge by its immediate effects. In these instances, it was so agreeable, without any after discomfort, that, when once given, the patients insisted on its continuance; and this is our guide.

The inhalation had better first be tried by the medical attendant himself; twenty minims being dropped on a handkerchief, and held before the mouth and nose at such a distance as to admit air, but not far off; and the patient directed to breathe naturally. If it irritate or nauseate, or be in any way repugnant, these are probably unfit cases. If it soothe, and the patient ask for it again, it may be very safely entrusted to a careful nurse or female member of the family, giving clear directions that at first no more than twenty minims should be poured on a handkerchief at a time, and held at a certain distance; and that the breathing and pulse should be watched. A second supply should not be used, if the first has produced ts soothing effect.

There is one question which must not be avoided, and that is the ethical one.

It is reported that Queen Maria Theresa said, in dying, "Give me no opiates; I would meet my God awake." And we meet with similar instances, where the patient sometimes, and more often his friends, think it is wrong, in a religious point of view, to give narcotics in dying. But it is not recommended for these "great hearts" to use chloroform, but for the weak ones, who suffer so terribly from the mental and bodily exhaustion of dying. The best answer to those who doubt its propriety on religious grounds, is in these letters from the relatives of two of these patients. One writes:

"For my mother, chloroform seemed to clear her intellect, and enabled her to speak with thankfulness and hope regarding her eternal interests, as she had not done for so long; and I cannot but think that more precise knowledge of its effects in individual cases would dissipate those anxious thoughts of friends on this, as on other points, to the benefit of all concerned."

The next letter is from the lady who watched the patient, to her father.

"I am quite aware of your dislike to anything like a sedative being used when a human being is passing from time to eternity. The free use of chloroform in my uncle's case was unavoidable. After once inhaling it, he would have it. But, had it been denied him in his last hours, the agony which he was suffering was so excruciating that consciousness as to everything but that, was gone. Chloroform stupifies for the time; but when its effects are past, the faculties are alive again, and the person able to attend to other things. Were I in attendance upon a dying person, I should prefer his having temporary relief from pain; as, if he had not, he would not, humanly speaking, be more able to attend to the things which concern his soul's salvation, than if he had not obtained relief from chloroform; or I would rather say, not so capable."

These letters are satisfactory answers to those who, in ease themselves, theoretically object to soothing others in the saddest form of distress; for they show that chloroform thus administered, by relieving restlessness and pain, renders the patient, in intervals of ease, more capable of normal thought and feeling. —*British Medical Journal, July* 7, 1866, *p.* 10.

85.—BELL'S PARALYSIS OF THE PORTIO DURA.

[The following case was treated at Sir Patrick Dun's Hospital, Dublin, by Professor Aquilla Smith, with a view to show the special therapeutic action of a single remedy. The patient was twenty-five years of age, and paralysis of the left portio dura had resulted from exposure to a cold dry wind.]

Appearance on admission. — There is the usual difference between the expression at the two sides of the face when the muscles are attempted to be brought into action, but not otherwise.

The mouth is drawn to the right side. The tongue is protruded to the same side; but there is neither numbness nor loss of taste. When he shuts his mouth and puffs out his cheek, the left cheek is more protuberant than the right. His speech is slightly affected. With regard to the liquids, especially the letter "M," he himself says that this difficulty was more marked a few days ago, particularly as to the word "improvement." As is usual, he can neither whistle, purse up his lips, nor spit out; neither can he raise transverse or vertical wrinkles on the forehead of the affected side. He also manifests inability to complete closure of the left eyelids. The sensibility to touch, when pinched, was found unimpaired.

February 20th. *Treatment.*—Local electrilisation with the induced current resorted to. Wet sponges, and also pointed metallic conductors, severally employed. The sensibility was found to be a little impaired; but there was no pain felt. The orbicularis palpebrarum muscle readily responded to the electric stimulus, and, after the application of the remedy, he was able to cover the eyeball with the lids, though previously unable to do so.

21st. He can now cause the lids to meet over the eyeball, and thinks there is some improvement in eating.

Treatment.—Electricity again applied by wet sponges, and this time also with marked effect, especially on the buccinator, orbicularis palpebrarum, corrugator, and occipito-frontalis muscles.

22nd. He now closes the lids perfectly; he can eat much better; and can wrinkle the left side of the forehead a little.

23rd. Marked improvement in the power of wrinkling the forehead; he now feels twitchings in the orbicularis palpebrarum.

24th. Improving in every way.

26th. Steady improvement; he can now move the mouth a little towards the affected side.

28th. Scarcely any distortion visible; when the muscles are brought into action he can almost whistle; the natural wrinkles are reappearing on the left side of the forehead.

March 2nd. The transverse wrinkles on the forehead are now well marked; the buccinator muscle is notably recovering its power. It is expected that this man will be discharged from hospital in a few days; and Dr. Smith looks on it as a striking instance of the curative effect of electricity independently of any other means.—*Medical Press and Circular, March* 1, 1866, *p.* 222.

86.—ON FEEDING BY THE VEINS.

By Dr. B. W. Richardson.

[At the meeting of the British Medical Association, at Chester, Dr. Richardson exhibited an instrument which he has invented for the transfusion of liquid into the veins. This liquid he proposes should be a solution of albumen, subjected for a long time to a degree of heat below the point of coagulation. Dr. Richardson believes that digestion in the stomach is to albumen simply the exposure to a certain degree of heat. It is almost impossible to obtain blood from human subjects. This plan has been tried in many cases of cholera with great success. Dr. Sibson said that "he joined the association in an admiration which he could not quite express, for the beauty, simplicity, and exceeding delicacy of the apparatus invented by Dr. Richardson." Dr. Richardson said :]

A patient had been kept alive three days by injection; but she died from the continued discharge. They wanted a fluid that would sustain the patient. As to albumen, it was either poisonous or nutritious. When raw, it did not mingle with the blood; and it had the same effect as scarlatina on the kidneys. A donkey, upon which he had tried it, died with the same symptoms as in albuminuria. But since then he had found that, by subjecting it for a long time to heat beneath the point of coagulation, none of those symptoms were produced.. He believed that the digestion in the stomach was, to albumen, simply exposure to a certain degree of heat. The subject was so profoundly grand in its relation to both physiology and pathology, that he would not venture to say more; but during the year, he hoped, a very great advance would be made in this direction.—*British Medical Journal, Aug.* 25, 1866, *p.* 232.

87.—AEROZOIDS.

Mr. Glaisher's "blue mist" seems likely to stir up a lively discussion as to its nature, and several suggestions of a totally different kind in reference to its cause have already been made. As its dependence upon the presence in the atmosphere to an unusual extent of minute vegetable germs has been hinted, it may be as well to point out that the spores of fungi are probably always present to a greater or less extent in the air, especially at this time of year. Spores and sporules are essentially ubiquitous, being readily carried by the wind, and transported from the spots where they are originally produced. Though they exist in the atmosphere in seasons when the public health is very good, yet it is remarkable that they *abound* coincidently with the prevalence of epidemic disease; we have it recorded that rust and mildew have sprung up so rapidly upon articles of food and garments as to have appeared to herald approaching plagues, and to have been mistaken for "blood rain," striking consternation and terror into the inhabitants of many a city. So-called "blood- rain" is said to have been the forerunner of the plague of Rome. The present year has been especially characterised by the prevalence of all kinds of rust, moulds, and mildews, upon vegetation in general, more or less all over the world, to a marked extent at the Cape and in Ireland. We therefore must expect to find the air at the present time thoroughly well-charged with the germs of minute vegetable life. Suppose it be shown that the blue mist is due to a special abundance of fungi elements, it does not follow that the phenomenon has to do with cholera. The co-existence of the "mist" and cholera may only be a coincidence. The same depressing influences that render the human family subject to epidemic diseases also affect vegetable life in general, and predispose it to the attack of parasites—a view which is held by many trustworthy observers. It would be strange indeed if there were not some relation between diseases of plants and animals. Aërozoids, we suspect, are, in the majority of instances, germs of the uredo and penicillium.—*Medical Times and Gazette, Sept.* 1, 1866, *p.* 228.

88.—THE DETECTION OF POISONOUS ALKALOIDS IN ORGANIC LIQUIDS.

The medical practitioner is sometimes called upon to examine organic liquids, such as beer, tea, vomited matter, &c., for poisonous organic substances; and since the extraction of them has always been an affair of some difficulty, a method recently described by Wagner in the *Zeitschrift für Analytische Chemie* may be of interest. Supposing the liquid to contain ·002 to ·004 grammes of alkaloid in about a litre measure, the following method is adopted :—The liquid is first diluted with twice its weight of water, and then two cubic centimetres of a solution of iodine in iodide of potassium (127 grammes of iodine to the litre) are added to it. This produces a deposit, which, when separated from the liquid, is dissolved in a dilute solution of dithionite of soda. The second solution is again treated with the iodine solution, and the resulting precipitate is this time dissolved in an excess of sulphurous acid. The solution thus produced, when freed from the sulphurous and hydriodic acids it contains, consists of the alkaloids sought for, in the state of sulphates, and quite free from organic matter. These may now be recognised by the usual tests.—*Lancet, Sept.* 8, 1866, *p.* 267.

89.—CASE OF PARACENTESIS PERICARDII.

Under the care of Dr. Clifford Allbutt, at the Leeds General Infirmary.

[This rare operation was lately performed at the Leeds Infirmary in a case of acute rheumatic pericarditis, and with perfect success. The patient was 26 years of age, and was admitted on September 18th, 1866.]

On admission he was suffering from very acute rheumatism, both muscular and arthritic, and there was considerable dyspnœa and oppression. On examination the pericardium was found to be much distended with fluid, and there was acute pain in the region of the heart. A blister over the heart was ordered, and full alkaline and opiate treatment.

On September 19th, about 11 30 p.m., Dr. Allbutt was hastily summoned to see the patient, who was found sitting up in bed, with his elbows on his knees, struggling for breath. He was covered with a cold copious sweat. The area of pericardial dulness was found to be considerably increased, occupying nearly the whole of the left chest in front. There was perfect resonance all over the left lung behind. The patient was clearly at the point of death, and Dr. Allbutt determined at once to ask Mr. Wheelhouse to tap the pericardium. Mr. Wheelhouse was, therefore, called in to see the patient.

The extent of the pericardial dulness was now accurately defined, and the probable position of the apex of the left ventricle and of the auricle was as far as possible ascertained. Mr. Wheelhouse determined to open the sac half an inch from the sternum on the left side, and opposite the upper margin of the costal cartilage of the fifth rib. He passed in a fine trocar, inclining it slightly upwards and inwards, so as to enter, if possible, opposite the centre of the left ventricle. He pushed it onwards until he could distinctly feel the movements of the heart with the instrument; and then, sheathing the point, he pushed the canula well up to the heart until he could both feel and see the impulse. The trocar was then wholly withdrawn, and the fluid allowed to escape. This it did in a steady stream at first, which soon subsided into a saltatory flow coincident with the heart's contractions. The fluid consisted of a pale-pink coagulable serum. On the whole, about two and a-half or three ounces escaped. During the operation the patient gradually obtained relief, and after the canula was withdrawn, the bed-rest was removed, and he was able to lie down. The breathing was now only 36 per minute, and he was able to speak a few words and express that he felt relieved. The pulse had lost its rapid and struggling character, and could easily be counted, its number being about 110. The area of dulness was very decidedly lessened, but it was not thought well to tease the patient again with a minute examination. Mr. Coleman was good enough to sit up all night with the patient, who passed it in tolerable comfort, though there were several threatenings of syncope, which were warded off by large and repeated doses of brandy: all other medicines were omitted.

Next day the cardiac dulness had not increased. On the evening of this day (September 22), the breathing again became more laboured, and considerable delirium came on. Another large blister was placed over the region of the heart, and half-a drachm of liquor morphiæ was given; ten drops were ordered to be repeated every six hours. A comfortable night was thus passed.

On the whole, the patient may be said to have steadily improved from this time, and on October 13th he was discharged cured. The pericardial dulness on his discharge was little, if any, beyond the normal extent. There was a loud blowing systolic murmur heard over the apex. — *Medical Times and Gazette, Nov.* 3, 1866, *p.* 474.

90.—ON THE TREATMENT OF ANEURISM BY ACUPRESSURE.

By Henry Lee, Esq., Surgeon to St. George's Hospital.

[It is generally thought that the primary object in the treatment of aneurism should be to retard the circulation of the blood through the tumour. The truth of this is, however, doubtful; for when circulation is entirely arrested by two ligatures placed a little way apart, hardly any coagulation takes place in the blood included between the two ligatures. Speaking of wounds of arteries, Mr. Lee continues :]

Neither slowness of motion of the blood, nor any large quantity of coagulum, is necessary for the due performance of the process of union. It takes place commonly and readily in veins even after a wound has been opened several times, without any obstruction to the circulation ; and all that is requisite in order that it may in like manner take place in arteries is that the circumstances may be similar. Thus it is not retardation of the circulation, nor the quantity of fibrin deposited, that is essential for the cure of an aneurism ; but some mode of preventing the impulse of blood upon the newly-formed adhesions : in other words, apposition and rest. These objects may be attained in various ways. Whether intentionally or not, all the operations for aneurism which have been introduced, since the severer operation of amputation, as recommended by Pott, and that of opening the sac as advised by the older surgeons have been abandoned, tend to favour these conditions. What has been termed the Hunterian operation does so in a marked degree : the coagulum is left undisturbed, and the impulse is taken off the injured vessel. Instrumental compression, which has lately been used with a considerable amount of success, has evidently the same effect. This practice, as old as the time of Hunter, failed at first from the fact that those who attempted it tried to stop the current of the circulation, and thus produced a degree of pain which it was impossible for the patient to bear. With digital pressure the effect is essentially the same, but the degree of compression must necessarily be constantly varying.

The treatment by flexion is a discovery due to Mr. Ernest Hart. The sac, and consequently its contents, are prevented from being distended by being pressed upon in all directions by surrounding structures. The impulse of the blood, by the compression of the tumour against the upper part of the artery, is at the same time diminished.

Thus in all these methods of treatment the same essential conditions are provided for—viz., rest, and apposition more or less direct of the diseased or divided coats of the artery. It is true that cures have been effected in other ways. Thus the coagulum has been rendered so firm by galvanic action, or by the injection of the perchloride of iron, that it has not yielded before the pulse of the heart ; and this is to do by artificial means that which is naturally done in animals. And instances where this mode of treatment has been successful does not in the least militate against the necessity of the two conditions upon which I have insisted.

Now, in the various plans of treatment which I have mentioned, we may observe a gradual progress towards the accomplishment of the same end by more simple means. The old operation involving the opening of the sac was succeeded by modifications of the Hunterian operation. This, in many cases, was superseded by various modes of compression ; and this again by the less painful and less tedious plan of acute flexion.

I have now the pleasure of submitting to the consideration of the profession what, I believe, may prove a still more simple and more certain plan of treatment, at least in some cases—viz., the treatment by acupressure. I cannot but remember that six years ago I had the pleasure of reading before the Medical Society of London a paper, which was subsequently published as a separate essay. In this I advocated acupressure in certain operations upon the veins—a mode of proceeding which I had at that time practised for a period of seven years. In performing the operation for varicocele it had occurred to me occasionally to wound one of the branches of the spermatic artery, which was not controlled by the needles already introduced. Arterial hemorrhage would then take place ; but this was always commanded by introducing another needle, so as to compress the bleeding vessel. During this period a case occurred which left a strong impression on my mind that the action of arteries in other situations might be more simply and more effectually controlled by acupressure than by other means.

A young man had a wound in the palm of his hand, from which there was a copious hemorrhage. Various attempts were made to secure the divided ends of the vessel. These all failed. The radial artery was tied, and the ulna artery was tied, and I believe ultimately that the brachial artery was tied ; but however this may have been, the arm was at length amputated. It occurred to me that the hemorrhage might have been restrained by means of needles passed through or underneath the bleeding vessels ; and this I mentioned to a friend at the time before the amputation was performed. That such a mode of treatment might be effectual in similar cases has since been fully shown by Sir James Simpson's admirable work on "Acupressure as a Means of arresting Surgical Hemorrhage."

I will now give a case in which I had an opportunity of putting this plan of treatment into practice in a case of traumatic aneurism.

Henry G——, aged nineteen, admitted into St. George's Hospital on the 16th of September, 1866. On the 9th of September he received a wound on the lower part of the left popliteal space from a sharp knife. The wound at the inner side of the leg passed obliquely outwards to the extent of an inch or more. There was a great deal of hemorrhage at the time, but this was stopped by a handkerchief being tied round the limb. The handkerchief was allowed to remain until the 12th, when it was removed, but again applied. There was not any hemorrhage at this time. Having removed the handkerchief on the 15th, the bleeding recurred. He was now admitted into the hospital. There was a tumour on the lower and rather to the inner side of the popliteal space about the size of a large chesnut. This could be felt and seen to distend with each arterial impulse. When the tumour was forcibly compressed by the thumb, the posterior tibial artery still pulsated. A consultation of the surgeons of the hospital was held upon the case, after which a long slender needle, previously made for the purpose of acupressure, was introduced immediately to the outside and above the tumour, which was at the same time pressed inward by the point of the finger. The needle was made to penetrate deeply into the popliteal space; its point was then turned inward, and brought out immediately behind the internal tuberosity of the tibia. From the grating sensation conveyed to the fingers at this time, the needle must have passed through some fibrous or tendinous structures. A small quantity of blood escaped at each aperture which the needle had made; this was not of a dark colour, and there was no evidence of any large vessel having been pierced. The pulsation in the tumour stopped immediately that the needle was introduced, but the pulsation in the posterior tibial artery in the lower part of the leg could still be distinctly felt. It appeared from this, and from the experiment previously made of compressing the artery, that the aneurism had arisen from a branch of the popliteal artery, and not from the popliteal itself, and that the acupressure needle must have passed between the main artery and the wounded branch. Although the tumour had ceased to beat, a piece of cork was placed immediately above it and a little to its outer side, and confined in its position by an elastic band passed over the extremities of the needle.

July 20th. There had been a little pain up the thigh, but none near the aneurism; no pulsation in the tumour.

22nd. The indiarubber band was removed, but the needle was allowed to remain. There was some very slight irritation at the points where the needle passed through the skin. There was no other pain or inconvenience. The skin of the leg for a considerable distance was still discoloured, presenting the appearance of having been bruised.

24th. The acupressure needle was removed (on the sixth day). Some serous fluid followed its extraction, and a very small quantity of blood. A pad of lint was now placed over the situation that the needle had occupied, and retained in its position by a bandage.

26th. The pad and bandage were removed. Some dark, grumous-looking fluid escaped from the original wound. No pulsation in the tumour, nor fresh hemorrhage.

27th. The bandage was again removed, and a smaller quantity of the same kind of fluid escaped from the wound as on the preceding day.

29th. The wound discharged only a very small quantity of serous fluid.

Oct. 2nd. There was now no discharge from the wound. Some thickening could be felt in the situation of the aneurism, but there was not the slightest pulsation. The pulsation in the posterior tibial artery continued natural. The skin of the leg still remained discoloured.

4th. Feels quite well, and is in no pain. The wound has nearly healed.

6th. Was allowed to get up.

13th. Has had no unfavourable symptom since the last report. The discoloration of the limb has disappeared. He left the hospital apparently quite well.

The patient presented himself at the hospital on the 20th, and again on the 27th, when, with the exception of the scars of the original wound and of the acupressure needle, the limb was perfectly in its natural condition.

This case occurred in a young man; it was a traumatic aneurism, and it was in a branch only of the main artery. It does not therefore follow that an ordinary aneurism of the popliteal artery would be cured by the same plan of treatment. On the other hand, it must be considered that this must have been a large branch, that it was very near its parent trunk, that it doubtless had its accompanying veins and nerves, and that these sustained no injury from the degree of pressure which was applied to them. Considering that an aneurism of this size and in this situation was so speedily and so completely cured by acupressure—considering that the degree of pressure required is not such as to stop the circulation, and taking into account the fact that an acupressure needle may at any moment be removed, the facts are, I think, sufficient to justify the trial of this mode of treatment in other cases of aneurism. Should increased experience confirm the *à priori* reasoning, there is, I think, little doubt but that this would be a simpler and more effectual way of preventing the arterial impulse than any other hitherto practised.

In conclusion, I may mention that the needles best adapted for compressing large arteries are curved, with rounded, not cutting points These, when used, should be held firmly in a handle which can easily be removed. With a needle of this kind it is not easy to wound a large vessel in the living body.

[At the meeting of the Medical Society of London, at which this paper was read, the following observations were made on the subject.]

Mr. LAWSON TATE had always employed acupressure, and thought the only danger connected with it was that of putting it at random under large vessels. You must dissect carefully down as for the application of ligature. The pressure is sometimes kept up too long, when suppuration may result; in the case of the femoral artery pressure for twenty-four hours is sufficient. Acupressure can be adopted in positions where ligature cannot be safely used. The essential condition for success is that the inner coat of the artery should *not* be lacerated, for if the two sides of the artery are kept together for a few hours they will unite. For six or seven years he had never used a ligature, and had never been troubled with secondary hemorrhage. The great danger lies in compressing veins, not nerves. In two dogs he allowed needles to remain twelve and eighteen hours in the femoral vein; both died of embolism.

Dr. MACKINLAY, of Paisley, in an aneurism of the brachial artery, had passed one needle above and the other below the aneurism. In two parallel cases, the one treated by acupressure and the other by ligature, the former did the better. Never once out of sixty to seventy cases had he found secondary hemorrhage to occur. He mentioned a remarkable case in which, after acupressure had been badly applied he was obliged to tie successively the femoral, the external iliac, and the common iliac, and the patient ultimately did well.

Mr. LEE replied, and remarked that it was more difficult than most persons supposed to wound a vein or artery with a moderately blunt instrument. If you dissect down to a vessel you are much more likely to have suppuration around it than by simply passing a needle under it. In the case brought before the society, the operation had not been done at random, but after a careful diagnosis. He denied, as being contrary to all experience in London, that the lining membranes of arteries and veins, when the two sides are brought into apposition, unite readily by adhesion, as stated by a former speaker.—*Lancet, Nov.* 10, 1866, *p.* 520.

91.—A CASE OF POISONING BY CHARCOAL VAPOUR.

Under the care of Dr. RADCLIFFE, at the Westminster Hospital.

We are indebted to Dr. Maclure, registrar to the hospital, for the following very interesting case:—

G P., aged 37, a stout, muscular man, was brought to the hospital at three p.m. on the 19th of May last, completely comatose, and incapable of being roused by any means. His breathing was laborious and stertorous—36 respirations per minute. The cheeks were flaccid and puffed out at each expiration. Every eighth or tenth inspiration was deeper than usual, and of a sighing character. A considerable quantity of frothy mucus collected about the lips. The countenance was pale, but not livid; the skin warm and moist; the pupils were fixed and dilated; pulse about 90, moderately strong. He remained quite insensible to such stimuli as galvanism or pinching; but about four hours after his admission he showed some signs of pain when his whiskers were pulled. He was placed near an open window, with free access of fresh air. A purgative enema was administered, and mustard poultices applied to his legs. As he was unable to swallow, nutrient enemata were administered during the night. Towards the following morning consciousness gradually returned; he began to speak, and wished to go home. On questioning him, it appeared that, being at the time in perfect health, he had, at midnight on the 18th, shut himself up in a small room, closing the window and keyhole of the door. He then lighted a pan of charcoal, which he placed at a little distance from the bed, but rather at a lower level, and lay down. At one o'clock he got up, stirred the embers, and returned to bed, having felt nothing particular in the interval. Soon afterwards he fell asleep, but without experiencing any uneasy sensations, and he remembered nothing until he awoke in the hospital, more than twenty-fours afterwards. He was discovered late in the morning of the 18th, and had been actively treated before he was brought to the hospital.

On the 21st he was quite sensible, and complained of some pain at the epigastrium. His tongue was dry, and covered with a white fur; breathing easy. His urine was retained and required the use of the catheter. On closer examination Dr. Radcliffe observed that there was some anæsthesia and loss of power in the lower limbs, and that reflex movements could scarcely be excited by tickling the soles of the feet. In the evening he complained of pain in the chest; there was some dyspnœa and cough, and frothy mucous expectoration. A blister was ordered to the chest in order to relieve these symptoms. His diet consisted of beef-tea and milk, with bread.

24th. The cough and dyspnœa are much better; the tongue is moist and cleaner; pulse 96; paralysis and anæsthesia much abated. He now complains of acute pain in the glans penis, which prevents his sleeping; and his urine sometimes dribbles away involuntarily, notwithstanding the daily use of the catheter. The urine is alkaline, and loaded with pus; specific gravity

1023. Under the microscope it exhibits pus-corpuscles with crystals of triple phosphate. He seems weak, and his pulse is 100, and not strong. Ordered six ounces of gin and half a drachm of tincture of hyoscyamus in one ounce of infusion of buchu every four hours.

The next day he was rather better, and continued so until the 30th, when he complained very much of pain in the glans penis, with frequent desire to micturate, passing only about half an ounce of urine at a time. The catheter was passed, and about twelve ounces of turbid purulent urine drawn off. Tongue furred and white; bowels confined. To have a warm hip-bath and a dose of house medicine; a suppository containing four grains of opium was administered after the bowels had acted.

On the 31st all the symptoms were much relieved; and from this time he gradually recovered. He remained in the hospital for several weeks, complaining chiefly of debility, but was ultimately discharged, completely recovered from the effects of the poison.

The paralytic symptoms and the strong tendency to catarrh of the mucous membranes, first of the lungs, and afterwards of the bladder, after the primary apoplectic condition had passed away, were remarkable points in the history of this case. Such consequences as these, Dr. Maclure says, are not described in the works of Christison or Taylor, or of various French writers on poisons in similar cases. The man stated that he was in perfect health at the time when he made the suicidal attempt; and there seems to be no other way of accounting for these inflammatory symptoms than the accumulation of the mixed gases in the blood. The patient does not either appear to have experienced the usual immediate effects of the inhalation of carbonic anhydride, or carbonic oxide, such as headache, giddiness, noise in the ears, &c., before becoming insensible. As the products of the combustion of charcoal may differ very much in consequence of variations in the supply of oxygen, it would seem that in this case the gases were either too much diluted with air to produce the ordinary effects, or that some of them (possibly carbonic oxide) may not have been formed.—*Lancet, July* 21, 1866, *p.* 66.

92.—ON THE ACTION OF MEDICINES AS INFLUENCING OXIDATION AND NUTRITION.

By Dr. H. Bence Jones, F.R.S., &c.

The first great division of medicines consist of those which directly or indirectly promote or retard oxidation.

Medicines that directly promote Oxidation.

1st. Iron is probably one of the most certain remedies we possess, and when used in right quantity and at the right time, it seems as though its progress could be watched and its benefit accurately determined by the improved colour of the blood.

When a soluble salt of iron is taken, in a few hours some part of it is converted into a sulphuret, or is reduced to the state of oxide in the bowels, and thus losing its solubility and power of diffusion, it is thrown out as perfectly useless as if it had never been taken.

Another part escaping precipitation remains dissolved, and passes in from seven to ten minutes, when the stomach is empty, into the urine, where it may be detected, partly oxidised, if capable of oxidation, by the ordinary chemical tests ; and this part also is of no use unless some local action of iron on the urinary passages or bladder is required.

A third part, instead of passing off in the urine, diffuses from the liquor sanguinis into every texture, and into the blood-globules and white corpuscles, making a greater formation of hæmatocrystallin, and thereby promoting that combination with protagon on which the production of new blood-globules depends.

These blood-globules exercise a chemical action on the oxygen of the air with which the membrane of the air vesicles transmits, and they and the fibrin together appropriate the incoming oxygen and carry it to the capillaries, whence it must diffuse into each structure to support the oxidation which takes place everywhere.

Hence, speaking generally, the more iron we absorb the more blood-globules we form, the richer also the muscles become in hæmatocrystallin, and the more oxygen is taken to the capillaries, and the more oxidation proceeds in the tissues and in the blood.

2ndly. Oxygen itself, and still more ozone, may be considered as direct promoters of oxidation. From the time of Dr. Beddoes and Mr. Boulton to our own time, by inhalation or imbibition, these remedies have been used, but hitherto there has been no general recognition of their worth at all comparable to that which exists regarding the value of iron. Theoretically, ozone, if not oxygen, ought to be a most potent remedy in the treatment of diseases arising from suboxidation, but as yet the right mode of using these agents, except as fresh air, has not been discovered, and probably until some means is found of keeping up the continuous action of these gases no considerable benefit will follow their use as medicines.

3rdly. Alkalies furnish out of the body and in the body the most marked evidence of assisting in oxidising actions.

Very many substances are included in the class of alkalies, and probably no two of them have the same degree of action, although all have a general resemblance. Different alkaline substances diffuse into the blood and into the textures at different rates, partly on account of difference in the power of diffusion, but partly also on account of the amount of similar substances already present in the tissues. Thus, for example, carbonate of soda, carbonate of potass, carbonate of lithia, and phosphate of soda will pass in and out of the textures at different rates, according to the amounts of these substances already present in the fluid that saturates these tissues. The greater the difference between the quantity of substance in solution on each side (inside and outside) of any membrane, the quicker will equilibrium by diffusion be produced, and the sooner will the greatest possible action of the substance be obtained; and when the equilibrium is produced no further passage of the substance into the membrane will take place.

In the Medical Times and Gazette for September, 1865, I have given some account of the action of alkalies or organic substances and of the rate of passage of some alkalies into and out of the textures; and from the experiments there related you will see that there is every reason to believe that alkalies do act most powerfully by directly promoting oxidation. Wherever they diffuse they chemically assist the action of the oxygen there, and ultimately lead to the formation of carbonic acid. Hence the most efficacious alkalies are those that pass in most readily, and can be taken in the least neutralised condition. Thus caustic alkalies are more potent than subcarbonates, and carbonates more potent than bicarbonates.

4thly. Chlorine, iodine, and bromine are substances closely related in chemical properties, and all promote oxidation in the body by taking hydrogen from water and thereby liberating oxygen. Thus these substances act even more strongly as oxidisers than oxygen itself acts. For example, solution of blue indigo may be exposed freely to the oxygen of the air without being oxidised; but on adding chlorine, iodine, or bromine to the solution, oxidation immediately occurs.

When combined with alkalies these substances have sometimes a very different effect from that which they possess when free, for iodine is to iodide of potassium as chlorine is to common salt. But, like common salt, iodide of potassium is decomposed by some organic acids; thus we see it is decomposed on the skin when used as a lotion or as ointment, and thus hydriodic acid may be liberated in the textures, and this acid sets free iodine when air or free oxygen is in contact with it. Hence, when the oxidising action is certainly required, chlorine, iodine, and bromine should be used in the free state, and the combinations of these substances with alkalies will not always produce the same amount of action as the free substances themselves are able to effect. That the neutral compounds possess strong actions is well known in medicine, and the power even of common salt to form chemical compounds with the innumerable neutral substances that occur in the body, as for example the compounds of salt and urea and salt and sugar, shows that the iodides and bromides, wherever they penetrate, act chemically (though not as oxidisers always) on a multitude of organic and inorganic substances with which they come into contact.

5thly. Nitrates, chlorates, permanganates, and iodates are occasionally given on the supposition that they liberate oxygen in the body, but there is as yet no proof that any of these substances can give rise to an increase of oxidation in the textures. Alkaline permanganates and nitrate of silver are so rapidly decomposed on touching any organic substance, that they must be deoxidised long before they can pass out of the blood into the textures. The permanganate of potass can scarcely have passed the lips before it has given up part of its oxygen to the mucus of the mouth. Hence, probably, the efficacy of all these substances is quite independent of their chemical power as oxidisers.

6thly. Many other substances when taken in any quantity have a strong chemical action—for example, salts of zinc, salts of copper, salts of antimony, and salts of mercury ; of all these, the salts of mercury are the most remarkable on account of the use that has been made of them in the treatment of almost every disease in England. Salts of mercury belong to the class of irritant poisons ; substances that act chemically on the organic matters with which they come in contact, causing increased chemical action. The more soluble the preparation, the more violent the action on the different organic and inorganic substances with which it comes in contact. The solubility of corrosive sublimate in a solution of albumen, and the solubility to a slight degree of calomel in common salt, probably is the cause why these substances are capable, the one quickly, the other slowly, of passing into every texture of the body, and when there, the same increased chemical action takes place as we see occurs in the gums as the earliest sign of the constitutional affection by mercury. This increased chemical action cannot occur without increased chemical oxidation taking place in the same part at the same time. So that mercury salts, instead of stopping inflammation, actually cause increased oxidation, and the efficacy of mercurials in inflammation depends on a power of exciting the secretions and of irritating the mucous membrane from the stomach downwards, by which the bile is made to pass into the duodenum and mucus into the intestine, and on a chemical action on fibrin and albumen which stops the coagulability of the liquor sanguinis, and renders effused fibrin less cohesive, and so more easily removed by absorption, than it otherwise would be. Its reputation as an antiphlogistic probably is greatly due to its combination with opium, of which it corrects the chief inconveniences—namely, stoppage of the action of the liver, bowels, and other organs.

7thly. Many organic substances—as, for example, croton oil, castor oil, elaterium, ipecacuan, jalap, aloes, and many others—contain some oily or extractive matter, which acts on the outer or inner skin, and when absorbed gives rise to increased chemical action, whence come heat and oxidation, and more rapid circulation and quicker nutritive changes. If the increased chemical action rises to a sufficient height, then the increased circulation causes mechanical obstruction and congestion, and inflammation is the final result.

8thly. Counter-irritation must be considered as an oxidising action. The simplest example is friction, which not only acts by promoting mechanically the circulation of the blood, but the increased heat from the mechanical action gives rise to increased molecular change; in other words, to increased chemical action. External heat in all its different methods of application or production—as, for example, poultices, fomentations, water-dressings, flannels, cottons—acts in the same way as an equivalent amount of mechanical action would do. Increased thermal action cannot exist without increased molecular action.

Electricity differs from mechanical, thermal, or photal motion in its capability of passing into each texture according to its conductivity or resistance. Hence this form of motion may penetrate far deeper into different structures and affect actions in parts of the body far beyond the reach of mechanical or of thermal force.

Medicines that indirectly promote Oxidation.

The causes that determine the force and frequency of the heart's contractions are so many and so complicated that it is with the greatest difficulty that the effect of medicines on the circulation can be determined by experiment.

Among these causes are, first, the action of the nerves; secondly, the action of the muscular structure itself; and, thirdly, the chemical and mechanical quality and quantity of the blood, and its relative proportion to the system of vessels in which it is contained.

The present state of our knowledge regarding the influence of the different nervous systems upon the circulation shows that at the centre and probably also at the periphery a highly complex nervous action, partly stimulating and partly checking the contractions of the heart and vessels, takes place.

That a stimulating and a checking action of the nerves on the heart exists, and is in the highest degree complex, is evident from the following facts:—

1stly. The heart, at least in cold-blooded animals, when removed from the body goes on beating. Hence there must exist in the heart itself a centre of rhythmical action, the seat of which must be placed in the ganglionic cells which are disseminated through the heart's substance.

2ndly. On cutting the cervical portion of the ninth pair of a warm-blooded animal, the frequency of the heart's pulsations is remarkably increased, and on tetanising the extremity of the nerve, which is still by its cardiac branches in connexion with the heart, its motion is slackened, and may even easily be stopped altogether. Hence the ninth pair must be regarded as the heart's inhibitory nerves.

The inhibitory action can also be exercised through the same nerves by stimulating the parts of the brain between the calamus scriptorius and the corpora quadrigemina from which these nerves arise, and this leads to the conclusion that there exists in this part of the brain a centre which incessantly controls by means of the ninth pair the rhythmical action of the centre in the heart itself.

But the perfection of the machine and the complexity of the nervous action do not end here.

3rdly. The action of the heart may also be stimulated by tetanising the cervical portion of the sympathetic nerve, and hence there is reason to believe that there is somewhere in the upper part of the spinal marrow a stimulating centre connected by the sympathetic fibres with the ganglionic cells of the heart.

4thly. On the other hand, experiments on the heart of cold-blooded animals removed from the body can only be explained by the assumption of an inhibitory centre, situated in the septum atriorum (or common substance of the two auricles in frogs). Thus, then, there exist at least four different centres of nervous action:—A stimulating (or musculo-motory, as it is called) centre in the heart, and an inhibitory centre there also, and these counteract each other. Then there is a similar pair of centres in the brain and spinal marrow, connected, apparently, with the corresponding centres in the heart by two sets of nerves, the ninth pair and the sympathetic.

A poisonous agent may be carried to and act on any of these four centres, and thereby stimulate or check the heart's motion. Thus, for example, digitaline does not act upon the heart itself or on the nerves in the heart, but upon the inhibitory centre in the brain; its effect is produced through the ninth pair, for when these are cut digitaline does not act.

Still further complicating the nervous action, the inhibitory centre in the brain is in connexion with the sympathetic nerve, so that the action of the heart can be stopped by reflex action of the sympathetic nerves. Thus Bernstein has shown that when the two trunks of the sympathetic nerve in the abdomen are isolated (as can easily be done in the frog) and tetanised, the heart is seen to stand still as long as the ninth pair are entire.

As the sympathetic nerve and the ninth pair regulate the action of the heart, so the same power of contracting and relaxing the muscular fibres of the blood-vessels in the sublingual salivary glands, according to the experiments of Claude Bernard, is exercised by the sympathetic nerve and the chorda tympani.

If the filaments of the sympathetic nerve alone as they enter the gland are tetanised, a highly concentrated saliva is secreted, and the blood flows out of the gland of a dark colour. If, on the contrary, the chorda tympani alone be tetanised, saliva is secreted copiously, containing no great proportion of solid matter, whilst the blood that flows out of the gland is brightly coloured, like arterial blood, and sometimes the arterial impulse can be distinctly traced beyond the gland into the veins.

These results, so far as the colour of the blood and the abnormal pulse in the veins, can only be explained by assuming that the small blood-vessels in the gland are caused to contract by tetanising the sympathetic nerve, and that they are caused to dilate by tetanising the chorda tympani.

It is possible that a similar arrangement prevails throughout the whole circulatory system, but hitherto the existence of nerves which cause the vessels in general to contract has alone been demonstrated.

The consideration of the causes that disturb the circulation becomes still more complicated when the power of the muscle itself and the action of the fluid that circulates in the vessels and the resistance at the periphery have to be taken into account.

The power of the muscle itself may be affected by medicines, by nutrition, or by resistance. For example, it is most probable that veratrine stops the action of the heart in a totally different manner from digilatine, by affecting the muscular structure of the heart itself without the intervention of the nerves.

Moreover, the quantity of the blood and the average or total pressure which exists in the whole or in a part only of the circulation have been proved materially to affect the heart's power and frequency of action. Whenever an abnormal resistance is set up in any part of the system, as is. for example, the case in so many of the affections of the different textures of the kidney comprised under the name of Bright's disease, the action of the heart is increased, just as the action of any voluntary muscle has been recently shown to be increased, by adding to the weight it has to lift; and whenever the proportion which the bulk of the blood should normally bear to the capacity of the vascular system is altered by the contraction of the small arteries, the increase of average pressure thus produced increases the frequency of the pulsation of the heart, as Ludwig and Thiry have demonstrated.

By chemical or mechanical action upon one, or more than one, of these different nervous or muscular centres of force, medicines may increase the frequency or the force of the contractions of the heart. In consequence, the blood will be sent more quickly through the capillaries, and greater rapidity of chemical circulation through the different textures will be produced, and from this increase of oxidation and nutrition will arise.

Dr. Traube's experiments on the effects of digitaline, nicotine, oxide of carbon, curare, and antiarin all show the exceeding complexity of the action of these substances upon the heart; but from such experiments we shall obtain the data for rightly estimating the action of remedies on the nerves, and through them on the processes of oxidation and nutrition.

Medicines that directly retard Oxidation.

In this class are included that most important list of remedies which formerly constituted the medicinal part of the antiphlogistic treatment.

1st. Vegetable salines and dilute organic and mineral acids. These constitute the most directly acting remedies for fever and inflammation which we possess.

The oxidation of vegetable salines I have brought before you in the second lecture, Medical Times and Gazette, February 25, 1865.

The effect of organic and mineral acids in so checking oxidation as to cause even the accumulation of fat is mentioned in the seventh lecture, Medical Times and Gazette, December 2, 1865.

The first action of dilute mineral acids, when the stomach is quite empty, is on the mucous membrane and on the muscular structure; the nerves are not affected by dilute acids. When taken in the very small doses usually given, the alkalescence of the blood must quickly prevent any continued action of the acid as such on the blood-globules, the secretions, or the textures; but, by however little the alkalescence of the blood is diminished, or the blood-globules destroyed, by so much must the process of oxidation in the textures be diminished also; and if very large quantities of mineral acids could be evolved anywhere, accumulation and formation of fat, and probably a saccharine condition of that part, would be produced. The action of phosphorus, arsenic, and antimony in producing fatty degeneration of the liver, kidneys, and pepsine glands of stomach, and in stopping the liver from forming glycogen, may partly depend on the production of the acids of these substances in these different places.

Some organic acids, as, for example, oxalic acid, act quite as strongly in neutralising alkalescence as the dilute mineral acids, and multitudes of these organic acids are formed in all the textures of the body, and that mineral acids and bile acids when in excess may even produce so-called fatty degeneration is rendered certain by the experiments of M. Kühne, Traube, Leyden, and Munk. Oxalic acid and the bile acids, in combination with alkalies, have also the power of stopping the muscular action of the heart, and bile acids also rapidly dissolve the blood-globules, and thus stop oxidation.

Even the syrup which enters into so many prescriptions must be considered in its action as equivalent to so much organic acid taken into the system, and temporarily at least neutralising so much alkalescence in the blood and in the fluid with which every texture is imbued.

Vegetable acid salts have always been considered as antiphlogistic remedies, and when oxidised to carbonates they may assist in the removal of organic impurities in the blood and textures by ultimately adding to the alkalescence of the liquor sanguinis, and thus promoting oxidation everywhere; but this action is entirely different from that of organic and mineral acids, which, by lessening the alkalescence of the blood, directly retard the process of oxidation.

2ndly. Preparations of lead. As acids are the antitheses of alkalies, so lead is the antithesis of iron. In the proceedings of the Royal Society, June 15, 1865, you will see some experiments on the rapidity with which, in guinea-pigs salts of thallium pass into every texture, and can be shown to be present after six hours even in the crystalline lens. Whether lead passes at the same rate or to the same distance may be doubted, but at least it can be seen in the gums and be detected in the muscles of the forearm, and from this there is every reason to believe that it diffuses widely into each texture, and acts there according to the chemical properties of the different textures and substances with which the lead salts come in contact.

The multitude of organic substances from albumen to sugar that are precipitated by lead-salts shows the innumerable chemical actions that they must occasion in the body. Probably they precipitate both the hæmoglobin of the blood-globules and muscles, and the protagon of the nerves and blood-globules. Certainly the most marked visible effects take place on muscles, nerves, and blood-globules. Combinations are formed which prevent further change, so that an arrest of action in the formation of muscles, nerves, and blood-globules, takes place, From the affections of the muscles and nerves paralysis and pain are produced, and from the affection of the blood-globules anæmia results, and this causes diminished oxidation in the tissues, and hence accumulation of urates in the textures is apt to occur, and though acute or chronic gout may follow, yet the increased chemical action is usually insufficient to remove the deposit which the arrest of oxidation continues to produce.

3rdly. Oxide of carbon displaces the oxygen in the blood-globules and forms a stable combination with the substance of the red corpuscles which is not destroyed by oxygen. Thus, carbonic oxide stops the passage of oxygen into the blood and causes the same symptoms of poisoning as are produced by the hindrance of oxygen from passing into the blood.

Sulphuretted hydrogen, H_2S, is decomposed by the ozone of the blood, the sulphur being precipitated whilst the hydrogen reduces the blood to a lower state of oxygenation. An excess of sulphuretted hydrogen destroys the hæmoglobin of the blood and thus gives rise to a deficiency of oxygen in the circulation.

Arseniated and antimoniated hydrogen, H_3As, H_3Sb, have nearly the same action as sulphuretted hydrogen.

Phosphoretted hydrogen, H_3P, reduces the blood, becoming phosphorous acid, PH_3O_3, and producing asphyxia from want of oxygen.

Oxide of nitrogen, NO, acts in the same way as oxide of carbon, forming at first NO_2.

4thly. Sulphurous acid and hydriodic acid are direct deoxidising agents, and though the chemical actions they produce in the body may be able to produce heat and force in the body, yet the deoxidising action tends to stop the changes of oxidation which are going on in the body. The well-known action of sulphurous acid on nitrous acids, as in the manufacture of sulphuric acid, need not here be dwelt on. As the sulphurous acid takes oxygen from the peroxide of nitrogen or hydrogen, so hydriodic acid, when near a substance that easily parts with its oxygen, reduces it, and by this action these substances can retard the changes which may be taking place in the body; moreover, by neutralising alkali and by attacking the blood-globules, these and other mineral acids retard oxidation, and, like phosphoric acid, they are capable of producing fatty degeneration.

5thly. Gallic acid and tannic acid are strongly deoxidising agents. When in contact with alkalies, as in the blood, they are capable of taking oxygen even from the blood-globules. They probably have no action on the nerves or muscles, exciting no contraction of the muscular structure. The action of tannin in precipitating albumen and in forming indefinite compounds with the skin, gives no explanation of its action in astringing the small blood-vessels. The property of gallic acid as an astringent requires also fresh experiments for the explanation of its action.

6thly. Rest general and local for muscles and nerves, and cold, must be considered as means directly hindering oxidation.

The appearance of the arcus senilis first where the eyelid prevents the action of light, and the fatty accumulation in unused parts, are examples of the chemical effect of rest.

Medicines that indirectly retard Oxidation.

As in the causes that indirectly promote oxidation, so in those that retard oxidation, three different actions must be distinguished:—1st. An action on the nerves; 2ndly. An action on the muscles; and 3rdly. An action resulting from the quantitative and qualitative relationship of the blood to the vessels.

1st. On the checking or controlling action on the nerves that influence the action of the heart and the capillaries.

Of all the actions that can be produced by chemical agents acting in the body, this is the most remarkable for its rapidity and intensity.

The effect of so comparatively uncomplex a body as prussic acid (C_2NH) in bringing all action to rest is almost too rapid to be investigated experimentally; but the effect of more slowly acting substances—as, for example, morphia—shows quite as distinctly the mode of action of some of the most important agents that can be employed as medicines.

If any extremely delicate test for morphia existed, it would probably be found to follow the same course in rapidity and extent of action as quinine; when taken on an empty stomach, in less than fifteen minutes it would be detectable in the blood; and in every texture of the body it would reach its maximum in from two to three hours; whilst there it would act according to its chemical properties and the properties of the different textures with which it was in contact. In twenty-four hours it would greatly decrease, and in seventy-two hours, by diffusion or oxidation, it would entirely disappear.

What the chemical action of alkaloids on nervous matter may be is not yet determined. In the intensely energetic action of ammonia on the functions of the nerves, destroying immediately the electrical and all other actions which the nerve can produce, there may be strong evidence of the kind of power which the large class of alkaloids possess in acting on protagon. Chemistry has hardly entered on this vast field of inquiry, although the researches of Kühne and Eckhard and others show that the effect of inorganic and organic acids, alkalies, neutral substances, and salts on nerve and muscle must be determined before any clear views of the action of remedies can be obtained.

As soon as the salts of morphia enter into every nervous filament a molecular motion between the protagon and salt of morphia takes place; the resultant substance must be far less sensitive, far less capable of other molecular motion, than before, and until the morphia is destroyed by oxidation or removed by diffusion the nerve cannot recover its former mobility.

All experiments lead to the supposition that different alkaloids affect different parts of the complex nervous systems in greater or lesser degrees. Strychnia will act chiefly on the medulla; curarine and conicine will act most on the motor nerves; atropine will paralyse the nerves that contract the iris, while essarine will paralyse the nerves that dilate the pupil. No experiments at present can reach the causes of these variations, and for them and for other alkaloids we must take the fact and long for the explanation which chemistry will some day give.

The dry tongue, the stoppage of the secretion of gastric juice, bile, intestinal fluid, and of urine, the paralysis of the nerves that dilate the iris, the loss of mental and nervous power after morphia, all these actions are evidence of the effect of morphia in stopping all chemical action and in lessening oxidation by acting on the nerves which lessen the circulation of the blood.

In this way opium is one of the most potent antiphlogistic remedies we possess, and to it probably the calomel and opium treatment of this century owes its great reputation.

Of all the remedies that control hemorrhage probably opium is one of the most potent. This is effected by the same mechanism acting through the same nerves that indirectly retard oxidation.

2ndly. Medicines that lessen the action of the muscles. Rest and cold are the most potent agents that can be mentioned here. Salts of potass, ammonia, and lead, and probably salts of zinc and baryta, and perhaps salts of antimony and veratrine, digitaline, and nicotine, may have a direct action on muscular substance, especially of the heart, rendering it less sensitive to nervous influence, and thus controlling its contractions and causing less active circulation, and thus indirectly retarding or stopping oxidation; but here also we must wait for further experiments before any clear ideas can be obtained.

3rdly. The qualitative and quantitative relation of the blood to the vessels can chiefly be affected in a negative sense by local or general bleeding, by dilutions, and by evacuations.

If now, then, we discard the last lingering trace of the theory of phlogiston, according to the present state of knowledge the antiphlogistic treatment is the retardation of chemical action; and as oxidation is the chief of all the chemical motions going on in the body, antiphlogistic treatment is the arrest of an excess of oxidation and of all the mechanical, chemical, and other actions which arise from it.

Medicines or Food that directly promote nutrition.

It is not possible to draw any distinction between food and medicine; for oxygen, and iron, and water, and common salt are as necessary food as starch and gluten or fat and albumen, and increased supply of any substance that can enter into the structure of any tissue as surely promotes nutrition as increased supply of air increases oxidation.

The most remarkable example of increased nutrition by medicine is seen in the increased formation of blood-globules when iron is taken. The formation of hæmoglobin implies the formation of hæmatin, and without iron no hæmatin is produced, so that iron may be regarded as one food of the blood-globules. When it is absorbed in greater quantities than the ordinary food supplies, increased formation of hæmatin occurs, and in cases of anæmia this increased formation may be watched and stopped when it is considered that sufficient red colouring matter has been produced.

The chemical process by which the iron helps to build up the hæmatin is quite as unknown to us as was a few years since the formation of urea or any other of the innumerable organic substances which chemistry is now able synthetically to construct; but that the metal itself and many of its salts will lead to the increased formation of new blood-globules is proved by the innumerable preparations of iron that are in use in medicine, no one of which can be said to have an undoubted superiority in the cure of anæmia.

Possibly, by giving ready-formed hæmatin or hæmoglobin, a more rapid formation of blood-globules will be obtained than when any other preparation of iron is given.

The second remedy I may mention is phosphate or carbonate of lime in promoting nutrition of the bones. In cases of rickets, by giving ivory shavings, by powdered bone itself, by calcined bone, by pure phosphate or carbonate of lime, increased formation of bony substance may be produced.

The third medicine is cod-liver oil. This remedy clearly shows the impossibility of distinguishing between food and medicine. Its chief ingredients increase the deposit of fat in the cellular tissue, and this nutritive action on the adipose tissue differs in no respect from that which might be obtained by turtle, or cream, or olive oil. It is, however, quite certain that many substances besides oily matter exist in the cod-liver oil, and to these intermixed substances the cod-liver oil probably owes its medicinal repute. M. Chevreul has found no less than twenty-nine substances in mutton suet.

Fourthly, it is highly probable that other nutritive substances might be found which will specially promote the nutrition of particular textures, the more nearly in chemical composition the nutritive substance agrees with the texture to be nourished. Thus, in failing supply of the whole blood, the whole blood may be renewed by transfusion. In wasting muscle from exercise or other cause, meat may be the special remedy. Gelatine may perhaps supply the wasting cellular tissue, and even the highest of all textures, the brain and nerve substance, may perhaps be best restored by food of which these substances form a part. That phosphorus assists in the formation of protagon, as iron does in the formation of blood-globules, is far from any direct proof. Far less probability exists that phosphoric acid or the salts of phosphoric acid—as phosphate of iron and strychnia—can be so decomposed, and the elements recombined in the body, that the phosphorus may promote directly the nutrition of the brain or spinal cord. Whatever good these medicines may do otherwise, they can hardly be of use in helping the formation of protagon, either in the nervous substance or in the blood-globules. Phosphorus or phosphorous acid can be shown by direct experiments to be oxidised into phosphoric acid ; and hence the phosphoric acid of our medicines, though it may leave the iron or strychnia in great part to combine with soda and other alkalies, and may keep continually passing into a more or less basic salt, yet will never lose its oxygen by reduction, never be capable of combining with carbon and hydrogen to form that phosphorised oil which exists in the nervous substance. The worth or the worthlessness of phosphorus or phosphates as nutriment for the nerves must be established by experiments which the chemist alone is capable of making.

Medicines that indirectly promote nutrition by, 1stly, increasing the action of the heart, and, 2ndly, by lessening the resistance in the capillaries.

The different ways in which the heart has its action increased I have already brought before you in speaking of medicines that indirectly promote oxidation. The exceedingly complex nervous system which makes the human machinery so far more perfect than any made by hand, renders it almost impossible to rightly estimate the way in which and by which different medicines acting upon the different nervous centres can cause increased action of the heart. It is not less difficult also to determine accurately the probable action of medicines upon the different nervous centres in lessening the resistance in the capillaries.

Besides these nervous actions there is also the action on the muscle itself by medicines, and by the work which it has to do, and by the qualitative and quantitative variations in the fluid which circulates in the heart and vessels.

All these different actions lead to the same result, and that is, increased flow of blood to the part; and whatever cause produces a flux of blood to any structure, in that structure increased nutritive action will take place. The Hunterian experiment on the growth of the spur of the cock when implanted in the comb is daily repeated wherever, by mechanical and chemical or any other force, the quantity of blood brought to any part is increased. From corns to cancers increased flow of blood causes increased growth of healthy and diseased structures; and in whatever way increased supply of blood to any part is occasioned, increased nutrition of that part or texture indirectly is produced.

Increased flow of blood to any part may be produced in at least four different ways by medicine.

First, there may be an action on the nerves which increase the action of the heart.

Secondly, there may be an action on the muscular structure of the heart itself.

Thirdly, there may be an action on the nerves that dilate the capillaries.

Fourthly, the quantity and quality of the blood in the heart and vessels may be improved, so that a smaller quantity may become equivalent to a larger quantity.

1st. Alcohol, ether and chloroform nitrous oxide, chloride of methyl, olefiant gas, before they are destroyed by the oxydising process going on in the body, have a very decided action on the heart through the nerves. This action on the nerves does not proceed from the action of the blood-globules, because the symptoms appear before any blood-globules are destroyed, because animals with white blood are affected by anæsthetics, and because animals, as frogs, the respiration of which is with difficulty interfered with, are easily and strongly affected by these substances.

The protagon of the nerves is readily acted on by these substances, and it is this same protagon in blood-globules that causes them also to be attacked and dissolved by these stimulants.

The first action of these substances, then, is a chemical combination with one ingredient of the nerves—this is the primary stimulating action; and as soon as this combination is formed the nerve is rendered less sensitive (perhaps by being made less capable of being acted on by oxygen than before), and until the foreign substance is removed by oxidation the nerve is checked in its action.

2ndly. Of all the medicines that stimulate the muscles, probably ammonia is one of the most remarkable. It has no exciting action on the nerves of motion, but almost immediately deprives them of the power of exerting motion, but it acts as an intense exciter to the muscles.

3rdly. Of medicines that dilate the capillaries—that is, according to M. Bernard, by paralysing the sympathetic, which constantly tends to close the capillaries. This dilatation may be done in two ways—by dividing the sympathetic nerve, or by acting reflexly or directly on the cerebro-spinal nerve.

In the submaxillary gland the vessels are relaxed and saliva flows when the superior cervical ganglion of the sympathetic is cut, when the lingual nerve is excited as by vinegar, or the chorda tympani is irritated. Curare paralyses all the motor nerves, and then secretion of saliva takes place.

4thly. Improvement in the quality of the blood is chiefly effected by iron and animal food; or the quantity in any part may be increased by friction or by other force.

Medicines that directly retard nutrition, either, 1st, by themselves entering into combination with the organic substances of which the textures are composed, by which the chemical changes that would otherwise occur are stopped, or, 2ndly, by the accumulation in the textures of any of the substances resulting from the chemical changes in the textures.

Among the many mineral substances which act in the first way lead is the chief, and probably zinc, silver, copper, arsenic, antimony, and even in some rare cases mercury, as, for example, in methyl-mercury, can also combine with nerves, muscles, skin, or other textures, rendering them incapable of going through changes on which their action depends.

Among the substances which act in the second way must be included all the different products from each texture in their downward course to carbonic acid, ammonia, water, and salts.

Even the accumulation of "ashes" may come to be impediments to the chemical changes on which nutrition depends, the old textures may cease to be removed, and "calcareous degeneration" may result.

When carbonic acid accumulates in the blood and textures from excessive carbonic acid in the air, it causes no convulsions, provided sufficient oxygen is at the same time supplied; but the carbonic acid passes from the blood to the nerves, and acts on the moving and on the regulating systems of nerves. At first the pressure of the blood is increased, and the frequency of the pulse becomes greater; then the regulating action prevails, the pulse becomes slower and larger; then the pressure sinks quickly and considerably. The regulating action is exhausted, and again the increase of pressure shows the excitement of the motor system remains. Soon the motor action in the medulla oblongata is lost, and that proceeding from the ganglia of the heart alone remains, and this gives only slight pressure, small and quick systole and diastole, and long intervening pauses. The action on the nerves of respiration brings the respiration as well as the heart to rest, less oxygen goes in, and more carbonic acid accumulates till the muscles stop.

With carbonic oxide the same phenomena are produced with the addition that an immediate action of carbonic oxide takes place on the hæmoglobin of the blood-globules, and probably also on the hæmoglobin of the muscles (Virch. *Archiv*, 1865). Carbonic oxide takes the place of the oxygen and hinders the escape of carbonic acid, so that not only a deficiency of oxygen but an accumulation of carbonic acid is produced by it, the blood-globules being made incapable of carrying on the escape of carbonic acid from the textures (Pokrowsky, *Archiv for Anatomie and Physiologie*, 1866, *p.* 59).

The organic acids are a next step nearer than carbonic acid to the textures; and if these accumulate in excess in the blood and textures, stoppage of oxidation and nutrition results.

Of all organic acids, the bile acids, even when combined with soda, have been proved by Kühne, Traube, and Hermann to have this action most decidedly, rapidly causing "fatty degeneration," dissolving the hæmoglobin, so that this constantly appears in the urine, and stopping the muscular action of the heart.

Other organic acid salts, as lactric, citric, tartaric, for the time they remain in the textures before they are thrown out or burnt, stop the changes going on in the textures, and thus retard nutrition.

Sugar or fat in quantity, when taken, not only may lead to excessive deposit of fat, but may stop the changes in the material that is being removed to allow of new matter taking its place, and thus these substances produce an arrest of nutrition, even whilst adding to the adipose deposit.

The nitrogenous substances that arise from the albuminous textures react also in stopping nutrition; in addition to a multitude of other chemical actions on different textures which these substances may possess. What the amount and nature of these substances may be, and what the difference of their actions, cannot at present be conjectured with any certainty.

Beginning with carbonate of ammonia, and then urea, there are substances like kreatinine, animal quinodine, and higher compounds of carbon, all of which, if they could be given as medicine, would check the formation of the downward products from albumen, and thus would retard nutrition. What other actions these substances possess in first combining with nerves and muscles, and then rendering them less active as long as the combination lasts, must be worked out by the most careful experiments; and among the multitude of substances that may ultimately prove to be formed in the body from albumen, perhaps we shall find the same poisons which, when procured from vegetables, we know produce the symptoms that constitute some of the diseases to which man is liable. Even animal strychnia and animal morphia may, like indigo, quinine, starch, or cellulose, be formed in the body by some modification of the process of repair.

Medicines that indirectly retard nutrition by lessening the action of the heart and increasing the resistance in the capillaries, by which actions the flow of blood through the textures is lessened.

Four different actions may be distinguished, by any of which nutrition may be lessened—first, by action on the nerves of the heart; secondly, by an action on the muscular structure of the heart; thirdly, by contraction of the capillaries in consequence of an action upon the sympathetic nerve; fourthly, by the reduction, quantitatively or qualitatively, of the state of the blood.

It is impossible at the end of this sketch to bring before you in any detail the progress of experimental inquiry abroad on some of these subjects. I can now do little more than give you the names of some of the substances and the kinds of action which they possess, and I must refer you to the original works for full details of the experiments that have been made except in the single case of potash salts, which I shall take solely because it gives a good example of the progress that may be expected in therapeutical knowledge from scientific research.

A medicine may act in only one, or in many, or in all the different ways that I have mentioned, and practically there is rarely a limited action, for the medicine passes into every structure, and acts according to the properties of the substances with which it comes in contact.

1st. Of the medicines that act on the nerves of the heart in stopping its action, and thus making the circulation of the blood slower, styrchnia is one of the most remarkable. When applied directly to the heart, it acts on the vagi nerves, lessens the frequency of the heart's action, and also on the ganglia. Nicotin acts on the vagi nerves alone. Conicine stops all the action in the peripheral nerves without affecting the muscular contractility. Digilaline first stimulates, and then paralyses the regulating nerves, and then stops the action of the muscle. Quinine first stimulates, and then lessens, the action of the motor nerves of the heart.

The difficulty of determining whether the retarded circulation depends upon an increase of the checking action of the nerves or upon a decrease of the motor action, whether in the heart itself or in the brain, is in many experiments exceedingly great, and, to add to the complication, a reflex action on the vagi nerves may produce the same effect as though a poison had been applied to the vagi nerves in the heart itself.

Thus, as I have already said, mechanical irritation of the sympathetic nerve in any part of the abdomen may be reflected by the vagi nerves to the heart and stop its action. Of this the occasional effect of a sudden blow on the solar plexus, or of cold water when drunk, are familiar, though happily rare, examples.

2ndly. Of the medicines that act chiefly on the muscular structure of the heart, veratria may be taken as the most remarkable: it stops the contractility of the heart, and colchicine has probably the same action; both may also act on the nerves or muscular structure of the small arteries, and thus alter the circulation through the capillaries.

The effects of salts of potassium, compared with the effects of sodium salts, show that salts of potassium act very strongly on muscular contractility. When salts of potassium are injected into the blood, the pulse falls rapidly, and the pressure of the blood, when accurately measured, rises for the first few minutes and then falls, and if the injection is then repeated the heart stops beating.

A muscle, by the application of potassium salts, may be rendered unexcitable to the stimulus of electricity, and it may be restored by treatment with sodium salts. A nerve, also, when treated with potassium salt, loses much quicker its power of exciting contractions in a muscle than when a similar nerve is treated with sodium salt. Even a nerve of sensation, as in a tooth, may lose its power when treated with potassium salt.

So remarkable are the phenomena that even a similarity has been stated to exist between the action of potassium salts and digitaline.

Microscopic observation shows that chloride of potassium may affect the blood-globules, making them contracted and granular, while chloride of sodium has no action of this kind.

3rdly. Of medicines that act on the capillaries morphia is the most striking. It acts also first as a stimulant, and secondly strongly retarding on the nerves of the heart.

4thly. The blood is made less energetic in quality or quantity by bleeding, starvation, and excess of water; by lead salts and excess of acids mineral and organic.

Finally. If you look for a moment to the second great division of the healing art, you will find that among other indications of treatment two principal objects always present themselves to the surgeon. These are MECHANICALLY to control the working and repair of the body. In other words, he endeavours, qualitatively or quantitatively, to regulate by mechanical means the oxidation and nutrition which take place in every structure of the body.

Of each of these actions I must limit myself to one striking illustration.

The surgeon promotes oxidation in croup by mechanically opening the trachea. He retards oxidation after mechanical or chemical injuries by bleeding, by cold, and by rest, by defence from irritating substances. He directly promotes nutrition by removing pressure, as in the operation for strangulated hernia, and he retards nutrition by pressure or ligature in cases of aneurism.

As in Medicine, so in Surgery; the actions of oxidation and nutrition are mutually dependent everywhere, and no separation of these two functions in any part of the body really exists; and although I have endeavoured in this sketch to bring each of them separately before you, I have done so only in order that I might at least give you a clear though most imperfect view of the two most important of the many chemical actions of medicines which are now being worked out by experimental research. The progress of all accurate knowledge of the actions of medicines depends now on exact chemical and physical experiments, and by the perfection of these alone will the practice of our profession lose its doubts and difficulties and disagreements and deceptions, and become esteemed by all as the art that can confer the highest benefit upon mankind.

Instead of being as formerly blind wielders of heavy clubs that may cure the disease or kill the patient; or instead of being as at present "judicious" or injudicious "bottle-holders," Physicians at some future time will estimate exactly the effect of the increased or diminished action of any one force upon all the other forces concerned in the production of general or local disease; and by adding to the resistance of one or more forces, or by liberating more energy by means of the powers that are latent in food and medicine, they will restore that equilibrium of action in the body upon which our health depends.—*Medical Times and Gazette, Sept.* 8 *and* 29, 1866, *pp.* 245 *and* 333.

93.—ON PROGNOSIS IN HEART-DISEASE.

By Dr. W. H. **Broadbent**, Assistant-Physician to St. Mary's and the Fever Hospitals.

In affections of the mitral valve, the effects of the derangement no longer fall upon the left ventricle, but on the auricle, lungs, and eventually on the right ventricle. When there is regurgitation through the orifice, it would seem, at first sight, that a certain increase of capacity would be needed to make up for this loss, and part of the force of the left ventricle will be wasted or misdirected in driving blood backwards as well as forwards. Primarily, however, there exists no mechanical cause of dilatation, and the provocation to hypertrophy is but slight. Even this will be wanting in obstructive disease. Accordingly, in mitral regurgitation, we do not find any considerable degree of dilatation, or hypertrophy in the left ventricle, and in mitral constriction there may be contraction. A moment's consideration will show that increased capacity and strength of this cavity would by no means have the same effect in mitral as in aortic regurgitation. In the latter condition, so long as the mitral valve is competent, the result is that a larger volume of blood is thrown into the aorta at each systole; in the former, the increased amount of blood contained by the dilated ventricle would be divided between the aorta and the auricle, and a part of the increased force would be expended on the lungs. The auricle, however, becomes dilated and hypertrophied, as a result of the backward pressure, and this extends backwards through the lungs, and is felt by the right side of the heart, which becomes hypertrophied, often to a remarkable degree. It is difficult to understand how the pulmonary vessels and capillaries can resist the tension to which they are exposed; but it cannot be doubted that the right ventricle, by the additional force it exercises, aids in supplying the left ventricle with blood. This I look upon as a compensatory action of considerable importance. The increased pressure will send the blood more rapidly through a constricted orifice, and will tend to diminish the amount of regurgitation when the valve is incompetent. The thin walls of the right ventricle, however, readily yield to a distending force; and a frequent consequence of this is regurgitation through the tricuspid orifice, a provision which postpones the occurrence of pulmonary apoplexy.

I think it will be evident from the considerations adduced, that the relations between valvular and associated structural alterations is one of cause and effect; and I am convinced that both will be better understood, and their bearing on prognosis better appreciated, if, instead of looking upon their combinations as valvular disease of altogether uncertain amount, complicated by hypertrophy and dilatation of independent and accidental origin, or the converse, the hypertrophy and dilatation are regarded as the direct results of the valvular affection, and as measuring the degree of mechanical difficulty occasioned by the obstruction or incompetence.

It further seems to me clear that, on the whole, these changes, dilatation as well as hypertrophy, tend to neutralise the mechanical obstruction resulting from the imperfect action of the altered valves, and are thus distinctly conservative.

A valvular murmur, then, accompanied by hypertrophy, or dilatation, or both, is still attended with greater danger than a similar murmur not so accompanied; not, however, because the hypertrophy and dilatation add new elements of danger, but because the valvular change causing the murmur gives rise also to mechanical difficulty of serious character in the one case, and not in the other.

If, in addition to the disease in the valves, there be degeneration of the muscular substance of the heart, the dilatation may be taken as expressing the relation between the mechanical difficulty and the power of the heart to cope with it.

At the point which we have now reached, we may consider the prognosis of those cases in which valvular murmur exists, but without any apparent effects, the health and strength remaining good; and it must be remembered that, in heart-disease especially, prognosis includes not only the signs of approaching evil, but the probabilities of continued immunity.

When with the valvular murmur there is no change in the form or volume of the heart, or derangement of its action, it may be concluded that the valvular mischief is slight and unimportant. If, further, it be known to be of old standing, and to have been caused by acute endocarditis, the probabilities are, that it will not shorten life, will give rise to no symptoms, and have no ill effect whatever on the health. If, on the other hand, the murmur be recent, and have come on late in life, the same hope cannot be held out; it may indicate incipient degenerative change, and the progress of this change will determine the future of the case. Careful observation of the murmur from time to time, and of the state of the heart's walls and cavities, will be required in order to arrive at a safe prognosis.

When the murmur is accompanied by evidence of structural alteration, a cause for this exists in mechanical difficulty occasioned by the change in the valve. The seat and character of this difficulty are to be taken into account, and the order of relative gravity of the different valvular affections must be borne in mind; but we look chiefly to the condition of the walls and cavities. If the amount of change be only moderate, and especially if hypertrophy predominate over dilatation, the patient, subject to the condition mentioned above respecting the stationary or progressive character of the disease of the valve, may have an indefinite term of life before him, untroubled by cardiac symptoms. But the existence of the hypertrophy shows the valvular lesion to be such as to interfere with the circulation; and it must be remembered that the compensatory arrangement may be easily disturbed. Precautions, therefore, must be taken against occurrences which would throw increased labour on the heart, such as overwork, exposure to cold, &c.; and, if complications arise, the extent of the hypertrophy or dilatation will form an important element in the estimation of the degree of danger, indicating, as it does, a pre-existing injurious tendency.

In addition to the structural condition of the walls and valves of the heart spoken of, there may be indications of its functional condition (the subject being still in the enjoyment of good health), which may be reassuring, or the reverse. It will confirm other favourable signs, if the heart's action be equal, regular, tranquil, and of moderate strength, and not readily excited to palpitation. If, on the other hand, slight causes be sufficient to give rise to hurried and violent action, or if habitually there be any considerable departure from the normal force or regularity, these are further symptoms of the serious character of the affection.

The prognosis becomes more grave with increased amount of structural change, and more especially as dilatation is associated with or predominates over, hypertrophy; but these conditions, even in an extreme degree, by no means threaten a speedy dissolution, and are not inconsistent with a prolonged and comfortable existence. There is, however, an increased liability to complications from comparatively slight causes; and sooner or later a time arrives when the heart is no longer equal to the work imposed on it, and symptoms of embarrassed circulation arise. These then form the next element of prognosis, and, when well marked, give more definite, though more unfavourable, indications. The period of time at which they follow the occurrence of the valvular lesion is a most important point to be ascertained. The more quickly they supervene, the more serious their significance. When they appear early, they show that the mechanical difficulty caused by the valvular incompetence has been too great for the compensating tendency of the structural changes, which thus cease to indicate its amount. They are dwelt upon with great force by Dr. Stokes, more particularly as he considers "that the number of cases in which we are warranted in making a special diagnosis of valvular disease is small." We judge of the functional as well as structural condition of the heart by its action as seen, felt, or heard, through the walls of the chest, both habitually and as influenced by various circumstances. We learn the state of the systemic circulation by the pulse, and by examining the veins and capillary circulation; and of the pulmonary circulation, by the degree of dyspnœa, and the readiness with which this is induced. But there may also be present some of the cardiac symptoms, uneasy sensation, oppression, or actual pain referred to the heart itself, low spirits and irritability of temper, pulmonary embarrassment in various degrees, from mere shortness of breath up to the terrible paroxysmal dyspnœa termed cardiac asthma, or there may be incipient dropsy. The complications which immediately threaten life are reserved for later consideration.

The heart and pulse are to be observed mainly with a view to the indications of sustained or failing power in the heart, and of sufficient or insufficient supply of blood in the arteries. Speaking generally, if the heart evince vigour without excitement, and a strong heart-beat be not contradicted by a weak pulse-wave, the effect on the circulation has not reached a point attended with immediate danger. But each of the different valvular diseases gives rise to a characteristic modifiation of the pulse, which must always be taken into consideration. Aortic constriction tends to render the pulse small, with a prolonged wave. Aortic regurgitation is associated with the well known visible and audible collapsing pulse. Mitral regurgitation gives rise to irregularity in force and rhythm both in the action of the heart and in the pulse at the wrist. The degree in which these peculiarities are manifested, especially when traced by the sphygmograph, may assist in estimating the amount of change in the valve; if they be not borne in mind, none but fallacious inferences would be drawn from the pulse in prognosis. When, in aortic obstruction, it becomes fluttering and irregular, and when the characteristic collapsing pulse of regurgitation ceases to be evident and is replaced by a pulse weak and frequent, the heart is failing, and unfavourable symptoms, if not present, may be expected. In mitral regurgitation, there may be extreme irregularity of the pulse; and there may be occasional contractions of the heart, which, from momentary weakness or from want of a sufficient amount of blood in the ventricles, do not reach the wrist, thus causing one kind of intermission. But so long as the arteries, are, on the whole, well filled by the systole, the heart retains a degree of vigour, and, in the absence of other unfavourable indications, may be expected to carry on the circulation indefinitely. It is, indeed, astonishing how little trouble may accompany mitral regurgitation, with enlargement of the right ventricle, and an irregular and intermitting pulse; and how long life may last. When it becomes weak, frequent, and fluttering, even though more regular, or when it is altogether uncertain, the imminence of dangerous consequences is great.

The occasional abortive systole referred to is commonly, if not always, the result of varying pressure on a dilated heart in the movements of respiration. In emphysema and bronchitis, the pulse is always weaker during the laboured inspiration, and stronger during the forcible expiration, as pressure is applied to, or withdrawn from, the heart; and a sustained powerful inspiration will in any person cause the pulse to be for the moment slow and feeble; but, when the heart is dilated and weak, it feels the effect even of ordinary respiratory movements, and frequently to such an extent that a systole corresponding with commencing inspiration, or with the respiratory pause, is so far neutralised by the removal of pressure as to fail in propelling the blood into the remote arteries; or, as it has seemed to me, the expansion of the chest has acted rather on the thin-walled left auricle, preventing the blood from entering the ventricle.

To return to our subject: whatever the structural condition of the heart and its habitual action may be, if it be liable to excitement from slight and varied causes, such as a little exertion, change of posture, moderate emotion, the taking of food or stimulants, this is an unfavourable sign.

Fulness of the veins indicates obstruction to the entry of blood into the right side of the heart; and this again usually implies obstruction to the pulmonary circulation. Pulsation in the large veins of the neck is the most reliable sign of tricuspid regurgitation—the last in the chain of consequences tending to dropsy.

In the state of the capillaries, we find most unmistakable evidence of an obstructed or stagnating circulation. The face congested in different degrees, the cheeks of a deep or dark red, or approaching to purple in hue, with blueness of the lips and lividity of the nose; or the face may have a dusky pallor, the nose being cold and livid. The extremities, again, may be cold and purple; the colour returning slowly after pressure. It is not necessary to go fully into the signs of sluggish capillary circulation, or to point out their significance. They belong in their marked form to a late stage of the disease, and their prognostic import is not to be mistaken.

The symptoms of secondary respiratory embarrassment are of every degree of intensity, and belong to every stage of valvular affections. Shortness of breath on exertion, and especially on walking up-hill or against the wind, is common to all heart-disease: and, except when experienced in an extreme degree, is not a very serious indication. It is simply an exaggeration of what occurs to every one taking violent exercise. The simultaneous pressure on the veins by all the muscles of the body at first brings the blood to the right side of the heart faster than it can be sent through the lungs and aërated—whence the panting. If the exertion be begun gradually, or be persevered with in spite of the dyspnœa, the circulation is equalised, and we "regain our wind." In valvular disease, this takes place with greater difficulty; but commonly, except in some cases, by starting gently and not giving in to the early dyspnœa, a sufferer from this may be equal to considerable exertion or sometimes even to severe manual labour. Shortness of breath to this degree is one of the symptoms often complained of for years without marked increase; and sufferers from it may have become so habituated to it, as to be unconscious of a condition of dyspnœa which at once attracts the attention of the physician. Frequently, however, "shortness of breath" and "want of breath," as it is often expressed by patients, becomes a most distressing symptom. The sufferer may have to stop and pant a score of times in walking one hundred yards; he may be unable to lie down; and may have paroxysms of breathlessness without assignable cause. In this case, the derangement of the pulmonary circulation is greater and danger imminent. Habitual cough, persistent bronchitis with watery expectoration, occasional œdema of the lungs, are signs that the obstruction is having its effect on the pulmonary structures, and premonition of what may be expected on the slightest occasion.

Commencing dropsy, which will most commonly be associated with, or preceded by, evidences of pulmonary obstruction, is a serious symptom, but of very different import in different cases. This, however, will be more fully entered into later.

It is not necessary to dwell on other symptoms, such as sleeplessness, or sleep broken by dreams, low spirits, anxiety, irritability, apprehension, which, while adding to the patient's sufferings and helping to wear out his strength, teach nothing in regard to prognosis not already known by surer signs. A careful study of the countenance cannot, however, be too strongly recommended. By certain indefinable tokens, with which we soon become familiar, we may often go in advance of more positive indications.

As to complications other than those in the lungs, whether secondary to the impeded circulation or originating in the organ affected, in the liver, gastro-intestinal tract, or kidneys, each must be set down as an unfavourable note. Most serious in renal disease, which at the same time deteriorates the blood, and imposes on the heart additional mechanical labour by obstruction in the systemic capillaries. The conjunction of cardiac and renal affections is most ominous.—*British Medical Journal, Aug.* 25, 1866, *p.* 214.

94.—EMPLOYMENT OF A NEW FEBRIFUGE—CHLORATE OF QUINIA.

By Dr. Lyons.—(Cases at the Richmond, Whitworth, and Hardwicke Hospitals, Dublin.)

Scarlatina Anginosa; Use of Chlorate of Quinia; Rapid Defervescence.—J. M., female, unmarried, aged 18, was admitted into hospital with well-marked scarlatina anginosa. The rash was very fully developed on the chest, arms, and legs, of uniform boiled-lobster tint; the pulse was 130 and very feeble, and the patient had a dull heavy look, and complained much of the throat. On examining the fauces, both tonsils, the velum palati, arches of the palate and back of the pharynx, were much engorged, of a deep claret colour, and much distress was experienced at any attempt to swallow. Both tonsils were enlarged and spongy, the crypts being filled with buff-coloured exudation, the right gland being specially engorged. The chlorate of quinia was ordered for this patient in three-grain doses every third hour; in addition to which the tonsils were washed with a 20-grain solution of nitrate of silver, and a gargle composed of chlorate of potash, perchloric acid, syrup and water, was directed to be used frequently during the day. Wine and beef-tea were also liberally allowed. On the following day a very marked improvement in the patient's condition was observable; the pulse had improved in volume and diminished considerably in frequency; the engorgement of the throat had much decreased, and deglutition was performed with far greater ease and freedom from pain. On the fourth day defervescence was well established; the pulse had fallen to 80, the throat was quite restored to a natural condition, and the patient was in all respects convalescent. In two other cases of scarlatina, but of milder form, the chlorate was employed with very satisfactory results.

Protracted Typhus Fever; Supervention of Diphtheria on the 21st day; Use of Chlorate of Quinia; Recovery.—This patient, aged 33, passed through almost all the possible conditions and complications of protracted low typhus. The maculæ became transformed into petechiæ, which continued persistent till towards the close of the case. Involuntary passage of urine and fæces, with diarrhœal discharges and much tympanitis, formed prominent features of the case for many days; days and nights passed without an hour's continuous sleep, or even rest; constant muttering, attempts to get out of bed, general nervous tremor with constant subsultus tendinum, and tossing of the head from side to side next supervened; the pulse became exceedingly rapid and feeble, and the heart's action assumed the *tic-tac* character, the impulse being imperceptible, and the first sound faint to the last degree of audibility. The most vigorous and well-sustained stimulation by day and night seemed alone to keep the feebly flickering flame of life from being at any moment extinguished. Later on suffocative catarrh was threatened and with difficulty averted, and the bronchial affection overcome. Finally, in the interval of the 21st and 22nd day of the disease, when some slight general amendment had been established, it was found that an extensive diphtheritic exudation of tough buff-coloured matter covered the hard and soft palate, the pillars of the fauces, the tonsils, and all parts of the pharynx within view, extending likewise to the base of the tongue, and forward on the dorsum of that organ to near the tip.

Turning the patient to a strong play of sunlight, fortunately at the time available, Dr. Lyons introduced the index finger of the right hand covered with an extemporized mop of old linen, and carefully swept off the pellicular exudation from all parts within reach; in some situations it was found so tough that the handle of a spoon had to be employed to detach it, as from the half arches of the palate and the base of the tongue. All parts within reach were then carefully mopped with the muriated tincture of iron by a piece of sponge of suitable dimensions tied on the end of an elastic sprig of wood. Dr. Lyons states that for his part he is an advocate for the careful removal when practicable of the pellicular exudation before the use of any topical applications. As in the case under consideration, the exudation, he affirms, often constitutes so complete and so impenetrable a coating to the mucous membrane, that it is, in his opinion, idle to expect beneficial result or any action whatsoever from the strongest local applications, which under these circumstances, cannot possibly reach the mucous surface. In the case in question the pellicle was fully detached by the means employed, the mucous surface laid bare being exceedingly vascular, and here and there showing bloody dots of minute ruptured vessels. The muriated tincture of iron was now freely applied directly upon the affected surface, besides which the throat was repeatedly gargled with a mixture containing chlorate of potash, perchloric acid and syrup, and with such decided results that no further exudation whatever took place. The patient was further directed to take the chlorate of quinia in about five-grain doses every third hour, according to the following formula:—

℞. Chloratis quiniæ, ʒiss.; acidi perchlorici, ʒii.; syrupi auranti, ℥ii.; aquæ destillatæ, ad ℥viii. M.
Sumat. ℥ss. 3tiis horis.

The gargle employed in this and the previous case consisted of two drachms of chlorate of potash, two drachms of perchloric acid, three ounces of syrup and five ounces of water. Under the use of these remedies the patient rapidly improved, and about the 28th day from the first invasion of the fever convalescence began to be fairly established, and continued permanent.

From the powerful oxidising and general stimulating agency of chloric acid, and the influence of quinia as a nervine-tonic, Dr. Lyons has been led to the idea of combining these two remedial agents with the view of obtaining a febrifuge medicine of great potency. Each atom of the chlorate will provide, it may be expected, five available atoms of oxygen from the chloric acid, chl. O_5, while in the perchloric acid, each atom contains seven of oxygen, chl. O_7.

From some half-dozen cases in which he has as yet employed this drug, including scarlatina, typhus, the diphtheritic case above mentioned, and low forms of pneumonia, Dr. Lyons has obtained results which so far satisfy him of its efficiency and utility, and he invites the co-operation of his professional brethren in testing the value of this salt of quinia in low pyrexial states.—*Med. Press and Circular, May* 30, 1866, *p.* 562.

95.—GUFFROY'S COD-LIVER EXTRACT.

The inventor of this preparation, which is coming into considerable use in London and elsewhere, proceeds on the hypothesis that the active medicinal principles of the cod-liver oil are to be found in the greatest abundance in the watery constituents or serum of the liver, which he has converted by his process of sugar-coating into an attractive preparation. He claims for his Cod-liver Dragées that they contain all the beneficial ingredients of full doses of cod-liver oil, and being of very small bulk and perfectly tasteless, they completely obviate the objections which bar the use of that agent in the cases where it is most wanted. We take our information from an explanatory pamphlet issued by the manufacturer, whose statements must stand *quantum valeat*:—

"Many persons, thinking that the nausea caused by the oil, was to be attributed to the colouring empyreumatic matters, contained in the common cod-liver oil of commerce, recommended the use of refined and colourless oils; but the slight advantage which was thus obtained in rendering the oil less repulsive, was by many thought to be more than counterbalanced by a loss of remedial virtue. Others, less happily inspired, proposed to administer the oil in the form of jelly, but that also failed. It was then attempted to render the oil less unpalatable by enclosing it in gelatine capsules, but little thereby was gained, since the gelatine in no way tended to promote the retention of the oil by the stomach. Some, again, have tried to produce an artificial cod-liver oil, by imitating its chemical composition. . . . . . It was evident that the composition of a complex organic product like the oil in question, could not be successfully imitated by any artificial compound. But it was equally clear that whatever virtues cod-liver oil possessed over other animal oils and fats, were referrible to the source from which that article was procured—namely, cod-liver. . . . . The inventors had noticed that the substances to which the best authorities attributed the medicinal properties of cod-liver oil —namely, gaduin, propylamine, iodides, bromides, and phosphates, were more soluble in water than oil; and as cod-liver contains a larger proportion of water than in oil, they formed the opinion that the watery components of the livers must carry off the greater part of those substances. Chemical analysis, followed by practical observations, fully confirmed these theoretical views. It has been distinctly proved that the oil contains but a very small part of the medicinal elements existing in cod-liver, and that the greater portion is left in solution in the waters which have hitherto been thrown away.

"This important discovery once made, it only remained to reduce the watery constituents of the livers to a proper officinal form, in order to possess a medicine containing the active principles of cod-liver, separate from the oil. That of extract was adopted as the most suitable. The waters were accordingly evaporated, and were found to yield a product which, on being analysed by Dr. Garreau, Professor of Chemistry in the University of Lille, proved to have the following important chemical constitution :—

Ichthyoglycine	50·000
Propylamine	2·545
Acetic, lactic, and butyric acids	6·000
Phosphoric acid	2·090
Sulphuric acid	0·200
Chlorine	1·525
Iodine	0·154
Bromine	trace.
Soda	1·170
Potash	0·211
Magnesia	0·366
Lime	0·510
Ammonia	2·862
Extractive matter undetermined, gaduin, &c.	10·620
Water and loss	21·747
	100·000

"The extract thus obtained presented, however, two difficulties: in the first place, it was found to be exceedingly deliquescent, and, secondly, it was too concentrated for direct administration. These obstacles were overcome by mixing intimately with the extract about an equal weight of cacao-butter. The resulting compound was a firm and stable mass, of which pills, capable of being sugar-coated, could be made. By these means the preparation assumed the form of sugar-plums or *Dragées.*

"A proper officinal form for administering the new medicine having been found, it was submitted to the Académie Impériale de Médecine of Paris, who, in the month of May, 1861, appointed a committee, composed of Drs. Bouillaud, Poggiale, and Devergie, to examine, experiment, and report upon it.

"At the meeting of the 21st October, 1862, the Academie approved and adopted the report of those gentlemen. It will thus be seen that the deliberations of the committee extended over a period of sixteen months—a length of time which afforded ample opportunity for prolonged trials and a well considered decision.

"This report, which is too lengthy to be here given *in extenso*, recognizes the accuracy of the analysis of Dr. Garreau of Lille, and deduces from it a comparison of the proportion of chemical elements contained in the extract, with that found in oil of the best quality. According to this comparison, cod-liver oil contains scarcely 3-1000th of the elements iodine, sulphur, chlorine, and phosphorus, while the extract possesses 98-1000th of the same elements, or their acids, thus showing thirty-three times the amount; and while, from the oil only, about 10-1000th extractive matter can be obtained, cod-liver extract yields 724-1000th, or seventy-four times more.

" 'If,' continues the report, 'chemical composition be taken as the measure of the comparative value of cod-liver oil and cod-liver extract, it must be admitted that twenty centigrammes represent nearly forty-five grammes of oil, or more than two tablespoonfuls . . . . . The extract obtained from cod livers contains, in a condensed form, all the active therapeutical principles of cod-liver oil. . . . The system under its use acquires more vigour, the appetite gradually returns, the complexion improves, and the muscular strength increases. In short, the administration of the extract, like that of the oil, promotes assimilation, and consequently tends to the improvement of the general health of the patient. The richness of its chemical composition, its efficiency and certainty of action on the economy, the possibility of administering it to the most delicate and fastidious persons, constitute it a valuable therapeutical agent in all those affections which require the employment of cod-liver oil.'

"Cod-liver extract has likewise been tried in Russia, under the auspices of Dr. Kalenitchenko, Professor of Medicine in the University of Karkow; and has been officially recognised and authorised by the Imperial Medical Council of St. Petersburg."

The course adopted by the patentee of this preparation is such as to merit, *ab initio*, the confidence of the profession. He has not desired or attempted—as others have done—to force the medicine amongst the general public, but has simply submitted it to the profession on its own merits. He expresses a hope that medical men will be induced to give it a fair trial; if they find it meet their expectations, prescribe it regularly; if not, reject it summarily.

With reference to its employment in practice, he says:—

"Possessing, perhaps, in a higher degree than cod-liver oil itself the peculiar remedial properties of that substance, it is well qualified to take its place in certain cases even when the oil can be borne. But irrespectively of its claims in this respect, the full value of which enlarged experience can alone determine, cod-liver extract specially recommends itself in the following circumstances:—

"1. When the oil is rejected by the stomach.

"2. When, although the oil is not actually refused by the stomach, the production of nausea and disgust, renders a change of remedies very desirable.

"3. When, without discontinuing the oil, there may be reason for wishing to increase its efficacy.

"4. When the oil, whether agreeing or not with the patient's stomach, has failed to produce beneficial results.

"These objects may, according to circumstances, be attained by prescribing the extract alone, by administering it along with some oily matter less nauseating than cod-liver oil, by alternating its administration with that of the oil, or by giving it at the same time with the oil, and thus, as it were, enriching the latter substance with a superadded dose of those more active principles, in which cod-liver oil is deficient."

We will not be expected to give an authoritative opinion here as to its merits. Theoretically it is simply a question of what the benefit derived may be due to. If it be simply the oil itself, it appears rather inconsistent to prescribe cod-liver instead of many less nauseating oils. If it be the Iodine, Ichthyoglycine, Propylamine, or other correlative ingredients, the inventor has, we think, proved his case.

We are not certain that Mr. Guffroy has been judicious in labouring so energetically to make the medicine pleasant to the taste, for the profession in Great Britain seem to look coldly on the Dragées and Troches of the French Pharmacy. If it is any inducement, we may say that the cod-liver extract, before it has received the coating of sugar, is as unpleasantly fragrant a preparation as could be desired.—*Medical Press and Circular, Aug.* 1, 1866, *p.* 127.

96.—ON THE TREATMENT OF GOUT BY HYDROCHLORIC ACID.

By Dr. James F. Duncan, Physician to the Adelaide Hospital.

[There are many points of resemblance between gout and rheumatism. Both are marked by high fever, by a painful affection of the joints, and by a tendency to sudden changes of situation in the seat of pain. Both diseases seem to depend upon the presence of an abnormal acid in the blood, the production of which in each instance depends upon imperfect oxidation of the blood and more or less impaired nervous energy as the cause of that imperfect oxidation. The presence of lithic acid in the blood, in gout, has been proved incontestibly by Dr. Garrod—and no one can doubt after the experiments made by Dr. Richardson, of London, that lactic acid is the efficient cause of the peculiar phenomena of acute rheumatism.]

Assuming for the present as a matter of fact that the blood in gout is charged with lithic acid to an extent far beyond the natural standard of health, and in rheumatism with lactic acid, let me proceed very briefly to state to you my views as to the production of these different conditions, and the principles of the treatment you have seen me use for their mitigation and removal.*

I shall not occupy your time with any detail as to the various theories which have been held by different authors on this subject, as you will find them sufficiently explained in the text books in common use. I shall only advert to that broached by Dr. Garrod in his recent work, which may justly be regarded as the standard authority of the present day. And while I feel that in venturing to differ from one enjoying so high a position as a practical physician and a man of science, I am taking a bold step, I feel at the same time that I would be wanting in my duty to you if I did not put you in possession of the opinions I have been led to form after careful study upon this subject, and of some of the facts upon which those opinions have been founded.

Dr. Garrod looks upon the kidney as an organ whose function is merely to separate from the blood certain principles which exist in that fluid already, and which having served their purpose in the economy, are no longer necessary to be retained. and which, if retained, would prove positively injurious. He does not consider that the kidney manufactures by its own inherent power these principles out of the blood, but that they are already in the blood in the form in which they come to be eliminated, and only require to be separated from the fluid in which they are dissolved by a sort of elective affinity. In other words, that the function of the kidney is one of excretion and not of secretion. Urea and uric acid are two of these principles.

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* See a paper on this subject in the Dublin Quarterly Journal for May, 1865.

Now, in reference to these two products, he considers that they have no necessary relation to each other, and that the power of excreting one may be maintained in full energy, while that of excreting the other may be diminished or arrested. In gout he considers that the excretion of lithic acid is impaired just as the excretion of urea is impaired in Bright's disease. The latter position is too well known to be disputed, and a careful examination of the cases in Dr. Garrod's work establishes beyond all question that the quantity of lithic acid excreted during the paroxysm of gout is, as a matter of fact, greatly diminished. The consequence is that the lithic acid, if its formation in the blood continues to take place, must rapidly accumulate in that fluid. Accumulating here it acts as a poison, and gives rise to all those symptoms which characterise the phenomena of a gouty paroxysm. (See Dr. Garrod's work, p. 339.) "Gout," to use his own words, "would thus appear partly to depend on a loss of power (temporary or permanent) of the acid excreting function of the kidneys; the premonitory symptoms, and those also which constitute the paroxysm arising from an excess of this acid in the blood, and from the effort to expel the materies morbi from the system. Any undue formation of this compound would favour the occurrence of the disease; and hence the connexion between gout and uric acid, gravel, and calculi; and also the influence of high living, wine, porter, want of exercise, &c., in inducing it."

The objection I take to this explanation is this, that while it may be sufficient to account for the symptoms after the paroxysm is formed, it gives us no help to explain the cause of the loss of that uric acid excreting function in the first instance.

Let us now proceed to set before you the views I have been led to form on this matter. Adopting the idea broached by Liebig that the protein compounds of the body in the disintegration of the effete tissues are converted through the agency of oxygen, first into uric acid and then into urea, as shown in the diagram (see next page), it will follow that if by any circumtances the necessary degree of oxidation be not reached, the metamorphosis will be arrested in the state of uric acid, and this substance

$$\left.\begin{array}{lrrrr} & \text{C.} & \text{N.} & \text{H.} & \text{O.}\\ \text{1 at protein} = & 48 + & 6 + & 36 + & 14\\ \text{91 at oxygen} = & & & & 91\\ & & & & \end{array}\right\} = \left\{\begin{array}{rrrrl} \text{C.} & \text{N.} & \text{H.} & \text{O.} & \\ 15 + & 6 + & 6 + & 9 = & 1\tfrac{1}{2} \text{ at uric acid.}\\ 33 & & & 66 = & 33 \text{ at carbonic acid.}\\ & & 30 + & 30 = & 30 \text{ water.}\end{array}\right.$$

$$\overline{48 + 6 + 36 + 105} \qquad \overline{48 + 6 + 36 + 105}$$

$$\left.\begin{array}{lrrrr} \text{1 at uric acid} = & 10 + & 4 + & 4 + & 6\\ \text{4 at water} = & & & 4 + & 4\\ \text{6 at oxygen} = & & & & 6 \end{array}\right\} = \left\{\begin{array}{rrrrl} 4 + & 4 + & 8 + & 4 = & 2 \text{ at urea.}\\ 6 + & & & 12 = & 6 \text{ carbonic acid.}\\ & & & & \end{array}\right.$$

$$\overline{10 + 4 + 8 + 16} \qquad \overline{10 + 4 + 8 + 16}$$

which ought to be met with in very small quantity, will be proportionately augmented. I take it for granted that the removal of the nitrogenous compounds from the body after they have served their purpose in the economy, is naturally and principally effected in the form of urea rather than of uric acid, both on account of the much greater quantity of urea habitually found in the urine and from its greater solubility.

But it is not only from the disintegration of the albuminous tissues that uric acid may be thus formed. It is obvious that precisely the same changes may take place in the primary assimilation of protein compounds used as food, and I need scarcely tell you that the great majority of animal substances used for food are really but modifications of the elementary compound which has been designated protein. We have hence naturally two sources of uric acid in the system, one which may be considered almost constant in its amount, arising from the metamorphosis of tissue undergoing disintegration, and another variable quantity arising from the greater or less amount of nitrogenized food consumed by each individual. And in proof of this I have only to refer you to the diagram on the wall, which shows the marked influence which diet has upon the function of the kidney, augmenting both the urea and the uric acid when the food is exclusively animal, diminishing them when it is partly animal and partly vegetable, diminishing them still more when it is exclusively vegetable, and making them least of all when a diet is selected from which every atom of nitrogen has been carefully excluded.

Now, I need scarcely remind you that gouty patients are met with principally among that class of society who feed well, consume much animal food of the richest and most nourishing kinds. Here, then, we have one source of the production of uric acid in excess. Not necessarily I admit, for if the person who indulges in food of this description takes much exercise in the open air, and pursues some laborious occupation to work off the nutriment as rapidly as it is taken, the balance between the functions may be preserved and no harm follow. But if, on the contrary, he be a person of studious and sedentary habits, who takes little exercise and is not much in the open air—in whom, in fact, the function of respiration is only kept up at an average amount, it is plain that the oxidation in the lungs being insufficient to convert all the uric acid which is formed into urea, an accumulation must take place, to prove a source of subsequent discomfort and disease. But this is not the only source of mischief in these cases. I conceive that one of the causes of the increased production of uric acid is to be found in the imperfect or insufficient manner in which the function of respiration is performed, owing to the disturbed state of the digestive system. This is a point altogether overlooked by Dr. Garrod, which, if there be any truth in the chemistry of vital processes, must be regarded as of the greatest importance. You all know that the pneumogastric nerve is distributed to the lungs and to the digestive organs, and that its integrity is equally necessary for the healthy performance of both sets of functions. This being so, it is not unreasonable to suppose that whatever tends to concentrate unduly the nervous energy of this nerve in one of these two directions must *pro tanto*, interfere with its efficiency in the discharge of its functions in the other. If the alimentary canal be overloaded at any time with food beyond what the gastric juice can conveniently assimilate, and still more if that food be of an indigestible quality, causing the process of digestion to be protracted and difficult, the function of respiration must suffer. And here I would remark that the only test of the efficiency with which the process of respiration is effected, is the amount of carbonic acid exhaled in a given time. It is quite possible for the mere act of respiration to be accelerated, while the proper duty of the lungs is not performed up to the standard of health. The chemical changes in the blood may not be consummated notwithstanding the increase in the frequency of the acts of inspiration and expiration. Indeed, so far is this the case, that that very increase may be only Nature's effort to compensate for defective results, just as we know that the heart, when weakened by disease, endeavours to make up for its impaired energy by a quicker circulation.

I am not aware that any experiments have been made to set this question at rest by ascertaining the amount of carbonic acid exhaled by the lungs when the stomach is loaded and when it is empty; but it could easily be accomplished, and I have no doubt what the result would prove. It is an ascertained fact, that when the body is exposed to a high temperature, as in a warm climate, the quantity of carbon thrown off by the lungs is diminished, and this is one of the causes of hepatic disease in tropical districts, because a greater amount of this substance is left for removal in the shape of bile in consequence of this diminished exhalation of carbonic acid; and the same thing happens as a consequence of using alcohol—a circumstance which, taken with what has been said, may help to explain the production of gout in persons of intemperate habits, especially those who use alcoholic liquors which contain various forms of vegetable extracts, because the latter disturb the function of digestion more than others that are only so many kinds of dilute alcohol. Now, it is scarcely necessary for me to prove that the function of digestion is very materially disturbed in every case of gout. Sometimes this disturbance seems to be merely the result of impaired nervous energy which shows itself in the secretion of an inferior quality of gastric juice, so that persons of abstemious habits are often martyrs to a disease that is commonly understood to be caused by indulging in the pleasures of the table, but which, in their case, owns no discreditable origin. But most frequently the fault is not in the organs that suffer, but in the bad usage they are exposed to, more work being thrown upon them in the quantity and quality of food taken than they are capable of performing. If proof of this position were needed, we have it in the foul breath, the loaded tongue, the nausea and sense of weight in the stomach, the flatulence that most gouty patients are subject to, and still more in the common interval observed to take place between the time at which an error of diet has been committed and the development of the actual paroxysm.

You will now, I dare say, easily be able to catch the views I have been led to entertain regarding this affection: a disordered digestion is the primum mobile of the whole train of morbid phenomena. This leads to imperfect performance of the function of respiration; this, again, to imperfect oxidation of the protein compounds in the blood, whether of primary or secondary origin; and the consequent accumulation of these products in the form of uric acid. This accumulation again leads to the special symptoms of the gouty paroxysm, general disturbance of the entire system, suspension of the function of the kidney, and further augmentation of the materies morbi in the blood by its non-elimination at the proper channel.

This, of course, is but a very imperfect sketch of the subject, but the time at our disposal does not admit of my entering upon the discussion more fully at present.

Now for the treatment you have seen me in the habit of pursuing in these cases. It is simply the free use of hydrochloric acid, either alone or differently combined. The principle upon which I suppose it to act is not that it supplies oxygen to the uric acid to convert it into urea, but that it increases the digestive power of the gastric-juice, enabling the stomach more readily and effectively to accomplish the process of assimilation, and so indirectly increasing the nervous energy as to effect more perfectly the vital changes in the chemical constitution of the molecules of the blood, which are necessary to prevent the occurrence of this disease.

It is scarcely neceasary for me to tell you that this remedy is entirely at variance with all the plans of treatment hitherto in use for the cure of gout. Alkalies, in some form or other, to neutralise the lithic acid, and especially those alkalies or alkaline earths, which, entering into combination with this acid, will make salts of ready solubility, have been the fundamental supports of every plan that has met the approval of the profession. Other remedies have been used, but they were either ancillary to alkalies as the sheet anchor, or were employed for some special purpose. I do not put forward the hydrochloric acid as a specific to be used under all circumstances and in every stage of the disease, neither would I mean thereby to exclude the use of such other agents as may be obviously demanded to meet special contingencies. All that I contend for is, that it seems to me to act more directly upon the primary cause of the disease and to hold out greater prospect of effecting a real cure than any antacid remedy I know of. I shall now give you the particulars of the last case we have had under our care, briefly condensing the notes made by my clinical clerk, Mr. Reilly, not to exhaust your patience by too great a detail. I do so with the more pleasure because of its undoubted character as a true case of gout, of the severity of the symptoms, and of the fact that he was put upon the use of the acid from the first, which gave him immediate and permanent relief.

George H., an Irishman by birth, who had passed most of his life in England, was admitted into the Adelaide Hospital, Dublin, March 8, 1866. He was thirty-seven years of age, by occupation a land-surveyor and leveller, and as such constantly exposed to the weather. He always lived well, eating meat and cheese at least three times a-day, and drank pretty freely, prefering ale to porter or spirits. He was quite free from disease until eight years previously, when, in consequence of a severe wetting, he was laid up for four weeks with swelled ankles, the joints of his toes especially, the great toes being also affected. He had three subsequent seizures, in which his feet and ankles alone were engaged. These occurred at intervals of three months; his knees afterwards became implicated, and later his wrists and fingers. Formerly he was accustomed to have an attack four times a-year, but latterly they had become much more frequent, supervening upon the least exposure to cold or damp. The joints were always of a bright red colour, and afterwards desquamated. The family history was perfectly healthy, and his digestive powers appeared unimpaired. Five days before his admission, in consequence of a wetting, he was again attacked, when all his joints were more or less affected,—his ankles, toes, wrists, and fingers being particularly implicated. These parts were hot, red, swollen, and very sensitive. He had to be carried up stairs to bed, being unable to stir either hand or foot; he was sweating profusely; his urine scanty and acid; his tongue white and furred; the bowels open; his pulse 90. Large tophi existed on each knuckle, which were very red and inflamed. Several well-marked concretions were observable on each ear, but they were not painful or tender to the touch.

℞. Acid. hydrochloric. dil. ʒiss.; sp. chloroformi, ʒii.; tinct. colchici, ʒi.; infus. cascarill. ad. ℥viii.
Sumat. ℥i., 3tiis. horis sing.

March 9th. Much better; countenance clearer, less expressive of suffering; slept well; pains much diminished, particularly in the ankles; pulse 72.

10th. Pains in wrist and fingers better; sweating continues.

11th. Still improving; quickly regaining the use of his wrist and fingers; no pain in the lower extremities, except on attempting to move them.

14th. All his joints free from pain; able to move without inconvenience, except from weakness in his ankles; sweating diminished; tophi uninflamed and getting smaller, though still slightly tender; pulse 62; tongue clean; appetite good; urine alkaline. Ointment of iodide of potass to be applied to tophi; continue his mixture.

24th. Quite well; able to walk and write with his usual ease; complained of weakness. Ordered quinine mixture, the hydrochloric acid being stopped.

25th. Pains again troublesome; bowels confined; tongue red. Omit quinine mixture; to have an oil draught.

27th. Left the hospital at his own request to follow his occupation.

This case did not appear to require any preliminary treatment before having recourse to the remedy which I here recommended. It was essentially one in which the vital energy was depressed where a tonic line of treatment was particularly called for, so far as I have yet had the opportunity of forming an opinion it is in cases of this class it is mostly eminently useful. Some persons may suppose that the improvement was due to the colchicum which, you know, is generally considered a specific in gout and which was combined with it. But without attempting to undervalue the utility of this much vaunted remedy, my experience from other cases in which the acid was tried alone leads me to conclude that a very important part of the benefit is to be attributed to the acid, and having already shown this in other cases when the acid was tried by itself, some of which were recently in the hospital, I do not see why I should throw overboard a combination of two useful medicines in a suitable case merely to establish the reputation of one of them, which happens to be less generally known. I also added spirit of chloroform as a carminative to check the tendency to the generation of flatus in the intestinal tract, and infusion of cascarilla as a general tonic. The only inconvenience he experienced from the treatment was a slight diarrhœa on the 20th of the month, which did not require a change of the medicine, but merely a reduction in the dose. Any of you who saw the helpless condition of the patient on his admission, the expression of pain depicted on his countenance, and the sensitiveness with which he shrunk from even an approach to the bed clothes near the inflamed parts, cannot fail to have been struck with the marked and immediate improvement under the treatment, and especially at the change in the appearace of the tophi from what they were at first, instead of being red and swollen they diminished in size, and lost all that angry look that they presented on admission. In conclusion, this I can say with confidence, the theory I now broach may be wrong, but the treatment is pre-eminently safe and beneficial.—*Medical Press and Circular, May* 2, 1866, *p.* 443.

97.—ON A CASE OF HEART-DISEASE.

By Dr. Hyde Salter, F.R.S., Physician to Charing Cross Hospital.

The patient was brought to the hospital suffering with extreme shortness of breath; he was only able to speak in short and broken sentences; had a mingled expression of anxiety and exhaustion; and a certain sallow, almost jaundiced, tinge in his skin. He gave us with some difficulty (partly from his breathlessness, partly from his drowsiness and exhaustion) the following history.

Robert Lunt, aged 35, single, by occupation a sailor, has enjoyed unbroken health up to fifteen or eighteen months ago, when he had rheumatic fever; it affected every joint in his body, and confined him to his bed for three weeks. During the attack, he suffered, for the first time in his life, from great palpitation of his heart, severe pain in the præcordial region, and dyspnœa. His heart beat so strongly that he could hear it as he lay. When the attack came on he was on board ship, outward bound on a voyage to Australia. After he got into warm latitudes he convalesced pretty quickly, and was gradually able to resume his duties. At Bombay, four months after, he was again attacked with palpitation; which, however, lasted only an hour. Up to three months ago, on the voyage home, his breath was pretty good, and he was able to climb the rigging without inconvenience; but since he has been home it has failed, and he has suffered from loss of appetite and languor. About six weeks ago shortness of breath came on, and has never left him since. The difficulty of breathing renders it impossible for him to lie down: as soon as he attempts his breath is gone, he feels choked, and is obliged immediately to sit up. It also prevents all sleep, even in the erect posture. For the last week, he states that he has not had a wink of sleep, day or night; and his friends corroborate his statement. He feels drowsy and sleepy—overwhelmed with sleep, in fact; but he cannot get a moment's slumber, on account of the dreadful breathlessness that immediately attacks him. Just as he is dozing off, he "chokes," as he expresses it, and is wide awake in a moment. The slightest exertion, too, aggravates the dyspnœa so much that it is insupportable; he cannot cross the room, cannot even get out of bed or into it, without assistance. The breathing is worse after food, proportionately to the quantity eaten. He has had no cough. A day or two after the shortness of breath came on he began to spit blood; the blood was clotted, black, and in pellets, unmixed with phlegm, and not frothy. He would spit it a dozen times, perhaps, in the twenty-four hours. Appetite bad; bowels confined; urine scanty, high coloured, and not albuminous.

On examining the chest we found crepitant ronchus in patches here and there, especially in the upper part of the right side in front. Breath-sounds in other respects perfectly natural. Heart-sounds natural, except that the first sound was dull and defective. Pulse 84; respiration 30.

Now, before going any further, let us just stop to inquire what diagnosis could be constructed out of these materials, and what diagnosis actually *was* arrived at.

In the first place we came to the conclusion that the heart was hypertrophied. Its action was heaving, the distribution of its impulse was extended, and the area of cardiac dulness was too wide; at the same time the first sound was dull and obscure; and this fact had all the more significance from the other facts—from the defective sound being associated with so strong an impulse and with a full pulse.

In the second place, we came to the conclusion that there *was* some valvular mischief, in spite of the absence of any *bruit*. And why did we come to this conclusion? Simply because the symptoms appeared all to start from rheumatic fever. The great value and indicativeness of this fact cannot be overestimated. The man had the first attacks of short breathing he had ever had in his life during the rheumatic fever: there can be no doubt from the symptom he suffered from—precordial pain and palpitation—that his heart was then affected. Since that time he has suffered from repeated attacks of palpitation, and now he has manifest symptoms of hypertrophy. Now, taking those two points—rheumatic fever as the cause and hypertrophy of the heart as the result—what is the *tertium quid* that connects them? If rheumatic fever produces hypertrophy of the heart, by what agency does it do so? The only way that I can conceive is by organically damaging the heart's valves and orifices. Unless the rheumatic fever leaves some organic trace behind it, one does not see why, or how, it should ultimately give rise to cardiac hypertrophy—the mere fact of rheumatic fever having occurred could not produce it. Taking for granted, therefore, that the hypertrophy of the heart was primarily due to the rheumatic fever, I could not but conceive that thereby valvular disease was inevitably implied. What, then, did this bring us to? Valvular disease without a *bruit*.

And this brings me to the third point in our gradual narrowing of the diagnosis; namely, What particular valvular disease was that most likely to exist here? what valvular disease would be most compatible with absence of *bruit*, and with the other symptoms of the case? We conclude it to be *mitral narrowing*. There can be no doubt that mitral narrowing often does exist to a considerable extent without giving rise to a murmur, if the narrowing be unaccompanied with irregularities or asperities of orifice; and this is probably due to the feebleness of the force by which the blood is impelled (auricular systole) and its consequent feeble murmur-generating power. There can be no doubt, too, I think, that mitral narrowing has a greater tendency than any other valvular lesion to give rise to hæmoptysis, also a feature of this case.

My diagnosis, then, was some smooth narrowing of the mitral orifice, sufficient to give rise to considerable obstruction to the transit of blood through the left side of the heart, but not sufficient to generate a *bruit*.

Now, I have been at pains to give you the steps of my diagnosis and my reasons for them; for I do believe that it is only in this way—by examples, by shewing you individual instances of the application of its rules—that the art of diagnosis can be taught.

Before I tell you what the *post mortem* examination revealed, let me just finish the history of the case, which may be done in a few words.

After his admission into the hospital, no mitigation took place in the patient's symptoms. The urgent dyspnœa, especially on attempting to lie down, to sleep, or to move, continued. Day after day and night after night, he got no sleep, though begging and praying for it. To his sleeplessness was added now increasing restlessness, which greatly aggravated his distress. He would not remain in the same position two minutes together. At one moment he was in bed, the next out of bed, the next in bed again. As this restlessness increased, his drowsiness became exchanged for an apparent incapacity for sleep, and his manner had something the aspect of that of a patient suffering from incipient *delirium tremens*. Indeed I have no doubt that his condition did approach to that of delirium tremens, and that his restlessness and vigilance were due to extreme exhaustion produced by the many exhausting influences that were telling upon him. There was the exhaustion of his sufferings, the exhaustion of his sleeplessness, the exhaustion of want of food (for he took nothing), and the exhaustion of the hæmoptysis. And to these was added a much more potent source of exhaustion, that I have not yet mentioned to you. After he had been in the hospital two or three days, he was attacked with profuse bleeding of the nose, which was so profuse and so ungovernable that, after many remedies had been tried and failed, both nostrils were obliged to be plugged. In spite of this he lost blood largely every day, and became quite blanched and anæmic. You must all of you remember the ghastly appearance of the man, with his haggard expression, his white face, his plugged nostrils, and blood-bedabbled linen. The nostrils remained plugged up to the time of his death, for every day the bleeding burst out afresh.

Now, what was the meaning of this profuse and persistent epistaxis? I am inclined to think that it bore the same interpretation as the hæmoptysis—that it was due to the giving way of vessels that had become congested and distended in consequence of the impediment at the heart retrograding upon them. You know how surely heart mischief in this way ultimately produces systemic venous stasis; indeed, that this and its attendant results constitute some of the most marked symptoms towards the close of cardiac cases. That the heart mischief had produced this result in our patient the œdema of his legs showed; and that the epistaxis, although due to the same cause as the hæmoptysis, did not supervene till some time after it, is quite explicable by the relative position in the circulation which the two seats of hemorrhage occupied; for it would take some time for the impediment first felt at the pulmonary vessels to retrograde as far as the systemic venules. It is very possible that the tendency of the vessels to give way under the increased pressure of their contents might have been in this case assisted by a hemorrhagic tendency, which I have over and over again seen associated with the rheumatic diathesis. I do not say that the hemorrhage was positively caused in the way that I suggest, but I think it extremely probable. Indeed, considering how easily the venous plexuses of the nasal mucous membrane give way, and what conspicuous evidence we often see of venous turgescence in heart-cases, it seems to me more remarkable that epistaxis should not oftener occur in such cases than that it ever should. I do not know, however, that I have ever seen bleeding of the nose mentioned as a recognised occurrence in the course of cardiac cases. If this man's epistaxis really depended on the impediment to the circulation that the heart-disease offered, it would explain why it so resisted all attempts to stop it, because it was the result of a persistent and constantly acting cause.

The closing scene of the case was in this wise. The night before last (having been thirteen days in the hospital), the man was seized, without having appeared worse on the previous day than usual, except that he was weaker, with a more violent paroxysm of dyspnœa than he had before experienced; he felt as if he was dying. Similar paroxysms recurred throughout the night, getting more and more severe; and at length in one of them he died.

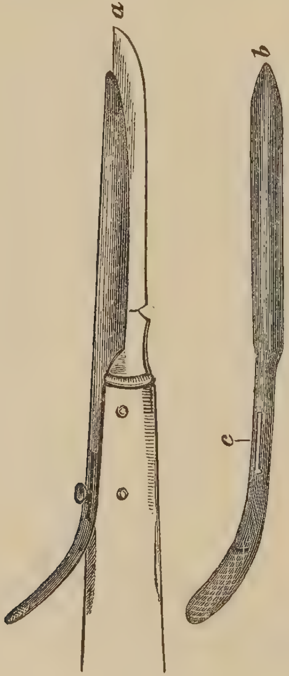
On making the *post mortem* examination just now, we found one of the largest hearts I ever saw; it might truly be called a "bovine" heart. It reminded one more of the ox-hearts we see hung up in butcher's shops than of anything human. The wall of the left ventricle was one inch and three-eighths thick; I measured it myself. The right ventricle was half an inch thick; and the auricles proportionately thickened. So far we found what we expected—the heart was, as we diagnosed, greatly hypertrophied. We then cut into it to see the valvular disease on which all this hypertrophy probably depended; and what did we find? *Every valve of the heart perfectly healthy!* There was nothing but a slight patch of opacity on one of the curtains of the mitral valve, that could never have interfered with its action. Now here was a strange thing!—perfectly compatible with the absence of *bruit*, and so far satisfactory but subversive of our hypothetical diagnosis, and leaving the hypertrophy utterly unexplained. Here was a case of enormous hypertrophy without valve-disease, without lung-disease (the lungs were perfectly healthy), and without kidney disease. I may mention, too, that the pericardium showed no trace of disease. Such cases are not common, but we sometimes see them, and they are among the puzzles of pathology. Perhaps the most curious thing is that, although the heart-symptoms that ended in this hypertrophy commenced with rheumatic fever, there should be no evidence of the ordinary agency by which rheumatic fever generates hypertrophy of the heart. Could it be that the patch of slight opacity on the mitral valve could be the remains and indication of a former valvular lesion sufficient when recent to have started the deranged action which gave rise to the hypertrophy? I cannot think it. The mind is baffled in seeking an intelligible solution of so difficult a problem.

In the way of treatment, there was little to be done. The great means of alleviating the cardiac embarrassment in cases of this kind—hydragogue catharsis—was in this case inadmissible from the man's exhaustion and the many weakening influences to which he was subjected. If we had dared to give him twenty grains of compound scammony powder every morning, and had thus draughted away from his bowels six or eight abundant watery stools, we should for the time have reduced the volume of the blood to be circulated and so disembarrassed his heart.

The only thing to be done, besides using every means to stop the hemorrhage, was to endeavour to keep him up in every way —by food and stimulants, especially alcoholic. Food we had the greatest difficulty in getting him to take. The condition of his circulation and respiration prevented our employing sedatives to procure him rest. Thus our hands were tied in every way, and there was little left for us to do, but to stand by and watch the inevitable and fatal issue.—*British Medical Journal, July* 28, 1866, *p.* 89.

98.—A NEW INSTRUMENT FOR TRACHEOTOMY.

We may notice an ingenious little instrument which has been contrived by Mr. WORTHINGTON, late House-Surgeon to the Middlesex Hospital. Its object is to facilitate what often proves one of the most difficult steps in tracheotomy — the introduction of the canula into the trachea. It consists of an ordinary scalpel, on the back of which lies a closely fitting, deeply grooved steel director (Fig. *a*). This director is adjusted by means of a slit (*c*), into which a button on the handle of the scalpel projects. During the early stages of the operation, and when the trachea is being opened, the director lies withdrawn upon the scalpel, as in Fig. *a*; but when the cut in the trachea has been made, and while the point of the scalpel is still in its canal the director is pushed down beyond the point of the scalpel and detached from it (which is done very easily when the dilatation in the slit *c* is opposite the button) and left in the trachea where it serves as a guide either for the introduction of the canula or for the subsequent enlargement of the cut in the trachea, should the one first made be of insufficient length. This instrument is so simple in its construction, and so easily managed, and aims at obviating what is often such a serious difficulty, that we have no doubt many surgeons would find advantage from its use.—*Medical Times and Gazette, July* 7, 1866, *p.* 8.



99.—ACTUAL CAUTERISATION WITH CHARCOAL PENCILS.

The *Répertoire de Pharmacie* states that it has occurred to several practitioners that the alarming apparatus necessary for actual cauterisation might, in certain cases, be replaced with advantage by small charcoal cylinders, which light and burn like a cigar. The lighted portion burns in an extent of four lines, and is terminated by a fine point whatever the shape and thickness of the pencil.

The pencil is sufficiently solid not to break, and emits no sparks, when perpendicularly applied to the surface ; if applied obliquely its resistance is of course less considerable.

The following is according to M. Bretonneau, a dispensing chemist of Paris, the most appropriate formula :—

℞. Pulv. carbon. populei, ʒjv.; potassæ nitratis, gr. xxx.; tragacanth, ʒj.; aquæ, ʒvj.

Prepare a mass to be rolled into cylinders, about four inches in length, and of the diameter of a common pencil. These pencils produce in burning but an insignificant amount of ash, which may be blown away in order to promote combustion.

We may observe that for upwards of thirty years, M. Guérin has used with much success for the same purpose agaric moxas of the size of common wafers.—*Journal of Practical Medicine and Surgery, April,* 1866, *p.* 160.

100.—THE PRESERVATION OF SULPHATE OF IRON.

Signor Pavisi recommends the following method of preserving sulphate of iron from oxidation :—Mix four parts of pure crystallized sulphate of iron, and an equal quantity of finely-powdered gum arabic, with distilled water, and evaporate the solution in a water-bath, at a low heat, till it has a sufficient consistency to be poured out on plates of glass. When it has been poured out in this way, and allowed to dry at a temperature of 30° cent. in the dark, it may be cut up into lozenges, which can be kept for any length of time in a coloured stoppered bottle.—*Lancet, June* 9, 1866, *p.* 633.

101.—OBSERVATIONS ON BRAIN SOFTENING, HEMIPLEGIA, &c.

By Dr. Alex. Robertson, Physician to the Town's Hospital and City Parochial Lunatic Asylum. Glasgow.

(Read before the Glasgow Medico-Chirurgical Society.)

That serious degeneration of a part of the body, along with partial or complete loss of function, result from sudden occlusion of the vessels on which it depends for its blood supply, and also that in some cases, though for the most part of a less formidable kind, these effects are produced by like obstruction to the return of blood from the affected organ, are facts with which the profession is familiar. Morbid changes due to the first of these causes, viz., arterial obstruction, have been observe in various organs, and tissues, as the brain, lungs, liver, kidneys, spleen, &c. The amount of structure involved, and the degree in which it is affected are of course greatly dependent on the size of the obstructed vessel, and on the completeness with which the part to which it is distributed is deprived of blood. Should the artery be a minor one, the portion of organ or tissue will be correspondingly small. Accordingly, minute hæmorrhagic, or fibrinous infarcta, or capillary embolias, as they are variously named, have been described by pathologists, more especially Virchow, Panum, Lancereaux, and others. Two great causes of occlusion exist. 1st. The vessel may have been rendered impervious by the impaction of a plug which has come from some other part of the vascular system, and become fixed at a particular point of its channel, though its walls at the seat of occlusion are perfectly sound; or, 2nd. arterial degeneration may exist in either of its two leading forms, the atheromatous, or calcareous; and, owing to the degree of its advancement, there may be ulceration of the lining membrane, or cicatrices, or perhaps a calcareous plate has become partially dislodged, and presents its free edge against the current of blood. It is evident that in such conditions deposition of fibrin upon the foreign body, as the affected point may be justly regarded, will readily take place, which, as it increases, will gradually block up the channel and obstruct the circulation. Embolism, as the first of these causes of obstruction is called, did not till of late years receive that attention to which it was entitled as a source of disease—sudden in its advent, and often most serious in its consequences. In surgical practice it by no means rarely has produced mortification and death of an extremity, necessitating its removal by amputation. Very generally in such cases the embolus comes from the imperfect valves of the left side of the heart, more especially the aortic; and, in the opinion of most pathologists, consists of fibrin which was originally deposited there, but has been set free by the force of the outgoing blood-current, and, after travelling along the larger trunks, has become impacted in one whose calibre will not permit its further progress. About five years since, in my own practice, in midsummer, I had occasion to remove the leg of a young woman, about twenty years of age, a little below the knee, on account of dry gangrene from the cause referred to. She died; and, at the *post-mortem* examination, the heart presented the appearances due to acute pericarditis and endocarditis. The aortic valves were loaded with fibrinous vegetations, some of which had doubtless been forced away by the impetus of the blood, and had ultimately blocked up the arteries of the leg.

In pregnancy and the puerperal state there is a tendency to hyperinosis or excess of fibrin in the blood, and a consequent proclivity to its coagulation within the vessels. Fortunately, however, the clots are usually formed in the venous trunks, and particularly those of the lower extremities, as occurs in *phlegmasia dolens*. They are generally also thrombotic in their nature; although sometimes it would seem that coagula so formed become separated, are carried to the right side of the heart, and, after leaving it,—probably having meanwhile increased in size,—are ultimately impacted in the pulmonary artery, producing most sudden and alarming symptoms.

Thrombosis has also received a vast deal of attention during the past few years. The first notice that I have been able to find of it is by the late Professor John Thomson, of Edinburgh, in Mr. Hodgson's work on *Diseases of the Arteries*, published in 1815. Dr. Thomson records very fully a case of extensive disease of the arteries, in which the leading trunks in the arm and leg were obliterated by plugs of lymph; and, owing to the deprivation of the blood-supply, the leg became gangrenous. It is somewhat surprising that the record of this very interesting case should not have directed general attention to the subject, and that not till Virchow and other pathologists had prominently noticed it was its importance fully realized.

As I have said, there are two leading forms of arterial degeneration, the atheromatous, and the calcareous, or ossification, as it is sometimes though erroneously called. Very generally in advanced life the aortic system of vessels presents indications of having undergone one or other of these changes. Calcification is for the most part confined to the old; atheroma is not infrequently met with in comparatively young persons. With regard to extent, neither form is, as a rule, limited to one or two vessels, but usually affects a large number. There are, however, very great differences, some arteries being much more involved than others. By and by I shall advert to this point in regard to one of its practical bearings. Owing to either or both of these forms of degeneration, the arterial wall is often rough and irregular, favouring the deposition of fibrin and the formation of plugs.

With these introductory observations I will now proceed to describe the case which I wish to submit to the Society.

Duncan M'Kenzie, aged 74, iron moulder. From the statements of his friends, it was ascertained that he had left his residence on the morning of the 26th of last March, about eleven o'clock, apparently in his ordinary health, which upon the whole was pretty good. He had not, however, walked many yards when he fell suddenly to the ground in a state of unconsciousness. On being raised, it was found that he was unable to stand, and that this was caused by loss of power of the left side of the body. Shortly afterwards he was carried into the Town's Hospital. At half-past three p.m., four and a-half hours after the occurrence, I found his condition to be as follows:—He lay in bed apparently unconscious; but, on asking his name, occupation, and putting other simple questions to him in a loud tone of voice, he answered in a confused manner, but yet quite correctly; and it was evident that he was at no loss for words, though his articulation was somewhat indistinct. The palsy of the left upper extremity was complete. The arm lay motionless by the side, the muscles being soft and relaxed, and he was quite unable to move it; nor was there the least involuntary movement on tickling or pinching the skin. He appeared to have a little power over the left lower extremity, although it was somewhat difficult to decide whether the slight attempts at withdrawal of the limb on irritation of the surface were to be ascribed more to voluntary action on sensation than to reflex contraction of the muscles. Sensibility on the affected side was greatly impaired, if not totally in abeyance. Thus, there was no indication of sensation on pinching or pricking with a needle the integument on the left side, either of face, arm, or side of body. However, as has just been said, there appeared to be imperfect sensibility of the affected leg. When the same tests were applied to any point on the right side, he quickly and clearly voluntarily withdrew the part, and more than once said, "Don't nip me." Pupils were nearly equally dilated, but not greatly so; they contracted, though somewhat sluggishly, under light. On opening the left eye and tickling the margin of the upper eyelid, there was no result; but on applying the same stimulus to the right eyelid, the eye was forcibly closed by instant and vigorous contraction of the orbicular muscular fibres, and he afterwards rubbed and pulled the eyelid with the right hand, as if to remove the irritating cause. He protruded the tongue when desired, but the point was directed towards the palsied side; the left angle of the mouth drooped, and the face was slightly drawn to the sound side. He swallowed with much difficulty. Respiration was unaffected, and there was no inequality in the movement of the two sides of the chest. The temperature was taken in the axilla with due precautions to insure accuracy. On the palsied side it was 98⅕; the sound one was a degree higher. The irritability of the muscles was tested by galvanism. A weak interrupted current was passed down both arms, producing contractions, which, so far as could be observed, were equal both in strength and rapidity in their respective muscles. Cardiac dulness slightly increased; sounds normal, except that the first was less clear than usual. Branches of temporal artery were prominent, and were also unusally firm under the finger. Pulse

76, regular, and of medium strength. Had incontinence of urine.

Observations of his case were recorded at intervals, until his death, which occurred at two a.m. on the morning of the 29th, being sixty-three hours after the seizure—but these need not now be stated, as the symptoms did not change materially in character, except that somnolency increased greatly during the last twenty-four hours of his life, and he died comatose.

The body was inspected on the third day after death. In making the examination, I had the valuable aid of my assistant, Dr. Barbour. The following were the morbid conditions:—1st, *Head.* Dura mater somewhat thickened, and adherent to the skull so firmly that a portion remained connected with the bone at the vertex, in removing the calvaria. Meningeal vessels contained a medium quantity of blood. All the arteries at the base of the brain were the seat of marked degeneration, principally though not exclusively of an atheromatous kind. The channel of some of the vessels was much encroached on by the morbid thickening. The entire cranial portion of the internal carotid artery was occluded by firmly coagulated blood, forming a complete mould of its interior. Similar obstructions also existed at the commencement of each of its principal divisions, the middle cerebral being more especially involved. On following the main trunk downwards in the neck, the plug was found to extend unbroken almost to the bifurcation of the common carotid artery. The early part of the vessel was highly atheromatous, and was distinctly dilated for about an inch. Here also a portion of the clot was found between its coats, the blood having apparently forced its way through a rent in the internal one. The cast of blood was of a deep red colour and of considerable consistence throughout its entire extent; but it was firmer at its commencement in the the neck than in the base of the skull. The whole of the right hemisphere of the brain, except the back part of the posterior lobe, and the convolutions of the upper surface, was distinctly softened. The degree of softening varied at different parts, being greatest in the middle lobe, below the level of the *corpus striatum* and *thalamus opticus.* The former of these centres was deeply involved; the latter was less affected. The softened parts were of a bright, pearly-white colour, and had a worm-eaten appearance. They presented no vascular points. The central part of the middle lobe, where the degenerative process was most advanced, was of the consistence of pap, but nowhere was the brain substance diffluent. The other hemisphere was normal in consistence, but the medullary substance had a yellowish tinge: consequently the contrast in the aspect of the two hemispheres was very striking. The specific gravity of the white substance just external to the *corpus straitum* was 1033 on the right side, and 1048 on the left.

Chest.—The heart weighed $13\frac{1}{2}$ oz. The left ventricle was dilated and its wall thin; the mitral and aortic valves were thickened at the base, and the curtains of the latter were less transparent than usual; but they all appeared competent for their functions, and the changes described were not greater than are usually met with in advanced life. The aorta was also the seat of morbid change, which was most marked in its abdominal part, especially about the bifurcation into the iliac trunks. Near the heart the wall of the vessel was soft, though presenting numerous atheromatous spots; but in the abdomen it was brittle, and at various points calcareous plates existed, covered by thin lining membrane. The cortical substance of the kidney had undergone fatty degeneration, but only to a limited extent.

Before entering on the diagnosis of this case, I will shortly direct attention to several of the leading symptoms, and *post-mortem* conditions.

1st. With respect to the sensibility of the palsied parts, considerable misconception has lately been shown by good observers in their statements regarding the degree in which it is affected in hemiplegia. In the remarks on a case of hemorrhage into the *corpus striatum* and *thalamus opticus*, accompanied by apoplectic symptoms, which I recorded in the Medical Times of the 15th of last March, I adverted to this point, and stated that the alleged escape of the sensory function did not consist with my experience in cerebral hemiplegia. That case was one of hemorrhage; the one which I now record is the effect of the other great cause of the apoplectic condition, softening: so that these cases confirm the old belief, that both sensation and motion are implicated, the latter certainly more uniformly than the former. They also indicate that the impairment or loss of these functions is dependent on the gravity of the structural change rather than on the nature of its cause. The mistake referred to probably arose from the fact that the sensibility of the palsied parts usually returns more quickly than the motor power; though I am satisfied from my own observations that in many cases of long standing, if careful examination be made by the compasses, according to Weber's method, it will be found to be only imperfectly restored. Exceptional cases, nevertheless, do occur, in which it escapes almost entirely, even when the motor function suffers severely, and others are met with more frequently where it is not greatly impaired. To what are we to ascribe this comparative immunity of the sensory function and also its more rapid restoration? The explanation is probably to be found in the observation of Brown-Séquard that sensory impressions continue to be transmitted by the central gray matter of the spinal cord, even though it be entirely cut through with the exception only of a narrow bridge; and, as Dr. Broadbent suggests in a valuable paper in the last number of the Medico-Chirurgical Review, we may fairly suppose that this apparently diffused transmission in the cord prevails in the higher part of the sensory tract, and is shared by the *thalamus opticus*, which is believed by many to be the principal seat of sensation. On this view, the property of transmitting impressions, unlike the motor function, is independent of the integrity of particular fibres, so that even though a large portion of the last named centre is destroyed, impressions which may arrive at it will be readily transferred to the part which remains sound; and therefore sensation will be rapidly restored.

In the description of the symptoms it was also noticed that there was loss, or, at least, great impairment of sensibility on the left side of the face. The degree in which several of the cranial nerves are involved in hemiplegia does not appear to have been at all carefully observed. Dr. Sanders, of Edinburgh, in a valuable paper in the Lancet, of October of last year, did good service in pointing out that the late Dr. Todd had fallen into error in ascribing the paralysis of certain muscles of the face in ordinary hemiplegia to functional lesion of the fifth pair instead of the facial motor; but strangely enough he himself has committed a mistake of a very similar nature, in doubting that in this form of palsy the fifth nerve is ever involved. His words are, "I have hitherto failed to observe any clinical evidence that the fifth pair is at all implicated in ordinary cases of facial hemiplegia," and he adds in a foot-note, "Romberg, however, describes motor and sensory paralysis of the fifth pair as present in recent cerebral hemorrhages. I know no evidence of this." During the last year I carefully noted the symptoms in forty cases of hemiplegia due to organic changes in the brain mostly of a hemorrhagic kind; and many of the patients are still resident in the Town's Hospital. In these cases the duration of the paralysis ranged from a few days to many years. On referring to my note book, I find that at the time of the examination sensibility was defective on the affected side of the face in no less than fourteen instances; two were hyper-sensitive, one had been overlooked, and in the remaining twenty-three its condition was normal, although not improbably, from the fact to which I have already referred, viz., the frequent rapid restoration of the sensory function, an impairment may have existed in some instances when the palsy occurred.

In connection with the motor function it was remarked that there was no irregularity in the movement of the chest; the intercostal muscles of the palsied side contracted as regularly and as vigorously as those of the sound one. This is usual in hemiplegia. The immunity of these muscles, and those also of the abdominal wall, besides a few others, seems somewhat anomalous, seeing that the muscles of the upper and lower extremities suffer so severely. Hitherto the only explanation given has been to the effect that the contractions of the muscles, which were little, if at all affected in hemiplegia, were of a *reflex* character, and that, therefore, organic changes in the *voluntary* centres of motion and sensation did not affect tham. This was satisfactory so far as the simply reflex movements were concerned, but it did not explain the continuance of contractions clearly in obedience to the will. Dr. Broadbent, in the paper to which I have already alluded, has suggested a very probable hypothesis in explanation of the difficulty. It is, that the nerve nuclei of those muscles on opposite sides of the body, which usually act in concert, or, as he has said, "whose action is bilateral, are so connected by commissural fibres as to be *pro tanto* a single nucleus." This combined nucleus, he considers, has a set of fibres from each *corpus striatum*—(which many physiologists concur with Dr. Carpenter in regarding as the great centre for originating voluntary motion)—and "is usually called into action by both of these centres, but will be capable of being excited by either singly, more or less completely, according as the commissural connection is more or less perfect." This transverse commissural connection has been observed in the case of the third nerves, and is therefore more than a mere hypothesis ; but further observation is required to establish its general existence.

No difference was observable in the contractions of the muscles of the two arms on the transmission of a galvanic current. Opinions very much opposed to each other have been advanced by leading authorities in regard to the degree in which the irritability of the palsied muscles is affected. Dr. Marshall Hall considered that they always responded more readily to galvanism than the sound ones ; but Dr. Todd showed that in many instances of paralysis from a cerebral cause the electric irritability is not increased. The view which at present receives the support of Duchenne, Meyer, and other high authorities on this subject is that, if the disease is in the spinal cord or nerves the muscular irritability is decidedly diminished ; but if in the brain, that that function is not usually impaired. Lately, Dr. Russell Reynolds has stated that his experience is opposed to the last conclusion, and says that in a very large majority of cases of cerebral hemiplegia he has found the irritability to be notably less in the paralysed limbs. My own impression, formed from the examination of the forty cases of hemiplegia above referred to and others is, that, as a rule irritability is dependent on, and proportionate to, the integrity of the muscular fibre. In cases of some standing it is well known, that from want of use and defective innervation, with its results, the muscles gradually degenerate. The sarcous elements become fatty, and are eventually removed, leaving little more than the sheath substance. Where degeneration exists, muscular irritability, as might be expected, is impaired ; but in recent cases, where no deterioration of the tissue has as yet taken place, I have been unable to observe any difference either in the strength or the rapidity of the contractions of the muscles of the two arms.

I referred particularly to the prominence and thickening of the walls of the temporal arteries. The importance of examining these vessels, in addition to the radial trunks has not been sufficiently insisted on. My observations have led me to place considerable value on a knowledge of their condition as a guide both to diagnosis and treatment. Arterial degeneration is, as we have seen, not confined to one vessel, but is generally widespread throughout the body. More especially, if we find one set of branches of a common trunk the seat of either form of degeneration, we may infer, with every probability of correctness, that other branches of the same trunk have undergone a similar change. Thus, should the temporal artery be unusually firm and prominent, and perhaps present irregular thickening of its walls on passing the finger along its course, we have evidence of morbid transformation in its coats. And it may further be inferred, as it and, say, the middle cerebral are branches of the one parent trunk—the common carotid, from which they are *mediately* derived, that this cerebral and other internal divisions are also in an unhealthy condition. I have examined after death several cases in which the radial and temporal arteries were hard and deficient in elasticity, and in every instance the branches of the basilar and internal carotid at the base of the brain were more or less diseased. It will be found, however, that generally the radial presents indications of degeneration sooner than the temporal, so that, in a given case, should the latter be affected, we may conclude with the greater certainty that the cerebral arteries are decidedly involved. It, however, by no means follows, though we may not observe any morbid alteration in these two vessels, that the arteries internal to the skull are consequently quite sound. Two instances in proof of the contrary were lately under my care. One of these was examined along with Dr. Barbour. In it the walls of the anterior temporal and radial arteries were found to be healthy, nor was there the least appearance of thickening of the latter at the point where the *superficialis volæ* arose abnormally as a considerable trunk, about two inches above the wrist, though atheromatous thickening is more apt to be found about a bifurcation than anywhere else. But on examining the basilar and branches of the internal carotid in the base of the skull, a number of yellow spots of that so-called deposit were found at various points in their coats.

In connection with the *post-mortem* appearances, it may naturally be asked why, when only one carotid artery was occluded, did such extreme softening occur, seeing that there have been numerous instances of ligature of that vessel, and even of both trunks, with due interval between the operations, without any material disturbance in the functions of the brain; though some have certainly terminated fatally through softening of the cerebral substance. The reply to this question is, 1st, that in the case which I have submitted to the Society, the arteries beyond the circle of Willis were plugged, so that the circulation could not be maintained in them through the branches communicating with the basilar behind, or the anterior cerebral of the opposite side in front: and, 2nd, all the other arteries of the brain were in a highly diseased state, and, consequently, could not accommodate themselves to an increased pressure of blood on their walls, so as to establish a collateral circulation. There was not the least evidence of even an attempt at the opening up of collateral channels. This is rather unusual, as in a large proportion of cases of brain softening indications of a compensating supply of blood are noticeable in the softening only of the central part of the area dependent on the occluded vessels, and also in the injection of the brain substance round the softened part. On the other hand, where the carotid has been ligatured successfully, no serious disease of the arteries has existed, and, in all probability there has been no obstruction to the circulation through the circle of Willis.

It was further stated that the internal carotid trunk was dilated for about an inch at its commencement. A writer in the Medical Times of the 30th of March last, states that he has noticed similar dilatation at the same points in numerous instances of atheromatous disease of the arteries. I have not yet had leisure to satisfy myself by examination of the general correctness of this observation; though it is so far corroborated by the case under consideration.

With respect to the nature of the softening, there can scarcely be a doubt that it was atrophic and not inflammatory. Not the least indication of increased vascularity existed; on the contrary, there was clear evidence of deprivation of blood in the obstruction to the circulation, and in the glistening-white aspect of the softened parts. Dr. Bennett, of Edinburgh, has certainly shown that in many cases of white softening, corpuscles are observed similar to those met with in inflammatory exudations; but, Virchow, who calls them granule-globules, maintains that they may be formed quite independently of inflammatory action, simply as a result of fatty degeneration. I regret that in my case no microscopical examination was made. However, the whole circumstances rendered it highly improbable that such action had existed.

The last point to which I wish to refer is the specific gravity of the brain substance. Its condition was very remarkable. So far as I can find, the greatest difference hitherto recorded in brain softening between corresponding parts of the two hemispheres is six degrees. Two cases are reported in which this difference existed—the one by Professor Aitken, of Netley Hospital, the other by Dr. Bucknill, now Chancery Visitor in Lunacy. In my case the softened part was no fewer than fifteen degrees less dense than the corresponding part of the opposite hemisphere.

We will now proceed to consider shortly some points in the diagnosis. In conversation with Dr. Barbour, previous to the autopsy, I remarked that the symptoms closely resembled those in the case of sanguineous apoplexy which I had just reported in the Medical Times and Gazette, and that, as in it we might probably find a hemorrhage limited very much to the *corpus striatum* and *thalamus opticus*. At the same time I added, that the pathological condition might be softening of the brain substance, as all the symptoms could as readily be accounted for, supposing it alone were present. Ought the diagnosis to have been more decided? was there any symptom which pointed clearly to softening rather than to cerebral hemorrhage? or ought the consideration of the case as a whole to have led us to that conclusion? There was no history of premonitory symptoms. Authorities are divided in their opinions regarding their greater frequency in softening than cerebral hemorrhage. In my own experience, I have not observed much difference in this respect, yet there has generally been noticeable a dissimilarity in the *nature* of the prodomata. In softening, some impairment of mind is often a prominent feature prior to the paralytic seizure; on the other hand, giddiness I have found to be the most frequent precursor of sanguineous apoplexy, while, as a rule, there has generally been little or no apparent defect in the mental powers.

The degree in which consciousness is affected is justly held to have an important bearing on the differential diagnosis. Dr. Todd insisted on its importance in this point of view; and Trousseau in his clinical lectures has also directed attention to its value as a symptom. They and other authorities are agreed, that when hemiplegia is *complete* and occurs suddenly, without loss of consciousness, softening of the brain may be diagnosed. There can be no doubt of the correctness of this observation. But my experience does not enable me to agree with Trousseau's conclusions concerning cases in which consciousness is partially affected along with complete hemiplegia. I quote his remarks on this point from the translation by Dr. Bazire,—"When the intellect is affected to some extent but not entirely—when there is obtuseness but not complete loss of sensibility—whilst there is absolute loss of motor power, as in the case of our patient in St. Agnes Ward, we must always, according to Récamier diagnose hemorrhage in connection with softening, or which has been termed capillary hemorrhage. I am too much a pupil of Récamier, I confess, not to adopt his conclusions, which my personal experience seems to me, besides, to have corroborated in the case I have just alluded to." These are his words. In the case which I have brought before the Society these very symptoms existed. The intellect was considerably affected but not entirely; there was also a slight indication of sensibility; there was absolute loss of motor power of both extremities on the one side (for the slight contraction of the muscles of the leg on pinching the integument can scarcely modify this remark); and the palsy occurred suddenly: yet, as I have said, there was not the least appearance of effusion of blood into the brain; on the contrary, the very opposite condition existed. On contrasting the symptoms with those existing in the case of sanguineous apoplexy, to which I have already referred, a very striking parallel will be noticed between them. Indeed, in all essential respects they were alike, so that, I think, I am warranted in submitting, in opposition to Trousseau's view, that general softening of a hemisphere, and a limited sanguineous effusion into the *thalamus opticus* and *corpus striatum* of one side may produce symptoms so similar as to render it impossible to distinguish between the two conditions.

The subject of treatment is so wide a one, that I shall content myself with making only a very few observations regarding it.

In my patient's case, after having cleared out the bowels with croton oil, I did little more than direct that milk and other fluid nutriment should be administered at short intervals. The idea of venesection or bleeding in any form was not entertained. The profession in this country is now very generally agreed on the impropriety of employing depletive measures in the majority of cases of apoplexy accompanied by palsy. It would, however, I think, be a mistake to refrain from bleeding in all cases. Without following any arbitrary rule, it is right to consider the peculiarities of each case, and thus ascertain the indications for treatment. It is of great importance to attend to the state of the heart and arteries, and I think, more especially the temporal trunks. Should the latter or the radial at the wrist be rigid, we may infer that the vessels inside the cranium are not sound, and that the supply of blood is at least not over plentiful; but should there be no valvular disease of the heart, should the left ventricle be healthy or hypertrophied without valvular affection, should there be no rigidity of the arteries, and the force of the circulation be good—in these circumstances abstraction of blood will probably be beneficial.

In the case of sanguineous apoplexy which I have spoken of, the temples were leeched; but afterwards I regretted, particularly in the light of the *post-mortem* examination, that I had not withdrawn blood from the arm. But I must not pursue this question further than simply to refer to Dr. Todd's work on the nervous system, in which indications for venesection and the treatment generally are clearly laid down. Dr. Chambers and others concur in the principles which he advocated. Trousseau has also an admirable clinical lecture on the subject. It is, however, I think, to be regretted, that he should have recommended venesection as a mere matter of expediency. Thus, after deprecating the employment of antiphlogistic measures in all cases of cerebral hemorrhage and apoplexy generally, as being not only useless but injurious, he adds (and I now use his own words) "to save your responsibility, avoiding at the same time what your conscience forbids, open a vein, but in such a way as only to draw an insignificant quantity of blood, and explain to the friends that it would be dangerous to go further." Making every allowance for the deep-rooted prejudices in favour of bleeding existing in France, in the minds of the general public and also of a large portion of the medical profession, I cannot help thinking that the counsel here given, and by a teacher of so great eminence, is much too accomodating in its character. Better far to exercise a little moral courage; frankly state our convictions to the friends of the sufferers, and even though, in an instance or two, unjust reflections be thrown on our treatment, before long, action consistent with the principles we hold is certain to gain general approbation, and, what is of still greater importance, will ensure the full approval of our minds.—*Glasgow Medical Journal, Aug.* 1866, *p.* 132.

102.—IODIZED COTTON.

By Dr. Robert Greenhalgh, Grosvenor-street, London.

[Iodized cotton is made as follows:—]

Two ounces of iodide of patassium and one ounce of iodine are dissolved in eight ounces of glycerine, in which solution eight ounces of cotton wool are thoroughly saturated and then carefully dried. The best method of applying it is to take a portion of the iodized cotton about the size of a half-crown piece secured by some silk thread tied crosswise, and, passing it through a speculum, to press it firmly against the cervix uteri, over which a piece of cotton wool similarly secured, somewhat larger, and freely saturated in glycerine, should be placed and retained *in situ* while the speculum is being withdrawn. It may be applied twice or three times a week, and be kept in the upper part of the vagina from twenty-four to forty-eight hours.

The cases in which I have found the application most useful are, subinvolution with or without congestion or induration of tissue ; in cases of chronic inflammatory enlargements and thickenings of the cervix uteri ; in one case of pruritus, apparently due to acid secretion passing through the os uteri ; in two cases of fibroid disease of the anterior lip of the uterus ; in chronic pelvic cellulitis ; in hæmatocele ; and in one case of epithelial cancer of the neck of the uterus.

It possesses the following advantages :—It is clean, light, and portable ; it produces no irritation ; destroys all fœtor ; is considerably stronger than the compound tincture of iodine, is more readily absorbed, and can be kept in contact with the diseased tissues for a longer period. Moreover, it does not soil the linen like the medicated pessaries and suppositories and many other topical applications in general use for uterine affections.

It was only the other day a clergyman's wife informed me that she preferred this local remedy to all others, because the laundress of the village, of which her husband is rector, does not know by her linen that she has any internal disease, which was always the case with former applications to that part. Several of my patients, wholly unconscious of the nature of the remedy, have complained of a taste of sea-weed from four to eight hours after its introduction. If the iodized cotton be withdrawn from the vagina in thirty hours it is nearly white, showing beyond all doubt that it has parted with the greater part, if not all, its iodine. On analysis, a portion of the iodized cotton which had been retained forty-eight hours in the vagina was found to contain only a slight trace of iodine. I should state that by the aid of the silk thread attached the patient can remove it herself at any time.

I am confident that if your readers engaged in the treatment of female affections will give it a fair trial in suitable cases, they will not be disappointed with its effects.—*Lancet, May* 26, 1866, *p.* 582.

103.—ON THE PRINCIPLES FOR THE SUCCESSFUL TREATMENT OF CANCER BY INJECTION OF ACETIC ACID.

By Dr. W. H. BROADBENT, Assistant Physician to St. Mary's Hospital.

[In an article published in the Medical Times and Gazette, priority is claimed for Sir James Simpson in the employment of various substances by way of injection, with a view to induce absorption or atrophy of malignant or other tumours. It appears that this is the case, although Dr. Broadbent was not aware of it until a few days before he read his paper before the British Medical Association. The priority which Dr. Broadbent desires to claim is that of having laid down the principles on which a successful treatment of cancer may be founded.]

It may be taken as demonstrated by the cancerous gland exhibited by Mr. Moore at the Pathological Society, that acetic acid injected into a malignant tumour may produce on the cancer structure *in situ* effects similar to those witnessed in cancer and other cells on the microscopic slide.

The case published by Mr. Moore in the Medical Times and Gazette of October 27 may further be taken as demonstrating at least the possibility of absorption of the tumour thus disintegrated—a result of which a single case of my own had merely raised a strong presumption, but of which I now have promise in more than one additional instance. It is satisfactory to have reached this point, and we may hope for repetition of these results with greater confidence, inasmuch as the conditions of their production approach those of an experiment in the laboratory; the acid is carried directly to the malignant textures, and has on them a certain chemical action from which the further consequences flow.

The points to be ascertained now are how this result may be obtained in the greatest number of cases, by what method it is to be sought in the various phases and stages of cancer, and what will be the ultimate effect. It was not my wish or intention again to write on this subject, except as a contributor of facts, until I had from experience some definite suggestions to offer as to the mode of procedure, and some definite evidence as to the value of the treatment. I think I have, however, reasons for departing from this resolution, since I hear of cases in which the course pursued is not such as I should suppose best adapted to secure the desired end, and anticipations are entertained which are inconsistent with the mode of action of the acid. In the absence of experience, which will require much time, the best guide will be a thorough apprehension of the principles on which we are acting; and a full discussion of these, which would have been premature until corroborated by experiment, may now be of service. Their application to various cases will be comparatively easy.

The chief points which constitute the adaptation of acetic acid for employment by injection against cancer are already sufficiently understood—its action on cells, and its non-coagulation of albumen. These relate mostly to its solvent effects on the tumour itself; the grounds on which absorption of the disintegrated products may be hoped for have not been so fully shown.

These will probably be best exemplified by a comparison between the action of acetic acid and that of caustics. These last owe their destructive effects on the tissues of the body to powerful chemical affinity with their constituents, with which, however, they do not combine as such, but which they change chemically, oxidising, dehydrating, or otherwise decomposing them. It is no longer albumen or gelatin which exists, but some derivative. This alone probably would prevent absorption. but the coagulation of albumen, which in most caustics is associated with the chemical change, seems to be an absolute bar to this process.

Again, affecting the constituents of the tissues chemically, they are indiscriminating in their effects, and though undoubtedly a malignant tumour yields more readily and perishes first, the surrounding parts are so far damaged that suppuration is provoked.

The basis of the action of acetic acid is, of course, chemical affinity, but this is not so powerful as to decompose the tissues, if indeed the tendency is in this direction at all. A piece of white fibrous tissue treated with dilute acetic acid swells up, becomes gelatinous, and loses entirely its fibrillated appearance; but by careful neutralisation it may be restored to nearly its original character and tenacity. Accordingly, when a little acid escapes into the connective tissue, there is smarting at the moment, and probably some inflammation afterwards. We may suppose that the white fibrous tissue has been partially gelatinised, but it is quickly restored by the alkaline blood to its normal condition. Cells thus treated are more seriously affected, the visible evidence of this being the clearing of the cell wall and contents, the change in the nucleus, and ultimately solution. This would seem to indicate a disturbance of those conditions on which the vital activity of the cell depends, and ceasing to grow and multiply, and failing to undergo development, they must undergo disintegration, even if not directly dissolved by the acid. The products of this process are so little different from ordinary plasma, and surrounding parts are so little affected, that absorption takes place.

The conclusions to be drawn from these considerations are, first, not to attempt too much at once. If a tumour of large size could be broken down by a large amount of acid introduced at one or many points, the amount of *débris* might be too large for absorption, and so much acid might pass into surrounding tissues as to excite inflammation, going on to suppuration.

Next, which is even more important, not to repeat the injections too soon. Absorption of such matters as result from the disintegration of a tumour is a slow process. On this point, too, we have the light of experience. In Mr. Moore's case the injection was made on September 5, and it is only on October 1 that the cancerous nodules are found to have disappeared. In the case of secondary cancer of axillary glands recorded in my pamphlet, the injection was made on June 26, and never repeated, and the the improvement has been slow but continuous ever since. The disease is not rapid, and we have ample time given us to oppose its advances. The rule to be laid down is at any rate to do no harm—least of all by precipitancy.

When the skin is involved, or when the tumour is of large size and rapid growth, my experience does not lead me to expect absorption. We may, however, cause it to be destroyed and discharged. It may or may not be found desirable to resort to this process largely, but there are certain special cases in which it seems to be the only treatment available. With destruction as the object, the indications become entirely different. The injection should be most thorough, and the acid strong, so that the entire disease may be removed at once. When the skin is broken, our best opportunity is gone; the acid escapes, and, not remaining in contact with the morbid structure sufficiently long, fails of its due effect.

Another question to which it seems to me necessary to refer in connexion with this treatment is the liability to recurrence. As the action of acetic acid is in no way specific, no specific results are to be expected—that is, no such results as are not clearly explicable and referable to the mode of operation of the remedy.

It is possible, perhaps probable, that there will be less liability to local return of the disease after removal of a tumour by acetic acid than by the knife, as the acid, being more or less diffused in the surrounding tissues, may reach germs which would have escaped the knife; and there is the more reason to hope that such will be the case from the excellent results which have followed the application of a solution of chloride of zinc after cutting operations as recommended by Mr. De Morgan. If, however, there are germs implanted in the vicinity of a tumour to which the acid does not penetrate, or if the local conditions which led to the development of the first manifestation of the disease still exist and are not modified by the action of the acid, a new tumour will appear. Local recurrence, therefore, must be expected in a certain proportion of cases, especially when of old standing; but as it will probably be detected early and at once dealt with, no fear need be entertained. So, again, if germs have been already disseminated, the removal of the original tumour will not prevent their development, but there is no danger that germs will be dislodged during the treatment, and set free to work mischief elsewhere.

We touch here upon a point where great divergence of views as to the ultimate results of treatment will exist, according as the doctrine of the constitutional or local origin of cancer is held. I personally look upon cancer as primarily a local error of nutrition, in which not the blood, but the part, is at fault. It may be said to be constitutional, just as almost every disease is, in a sense, constitutional; that is, there is a reason why, under similar circumstances, one person has it and another has not: but I do not believe in any condition to which the term "diathesis" is applicable, except such as is created by the existence of a tumour as a centre of dissemination. Facts, however, and not opinions, are what are required at the present moment; the value of the treatment must be determined by experiment, not by discussion, and I have, therefore, refrained hitherto from even a reference to this subject, and now forbear to enter upon it. But whatever opinions may be entertained on the point of doctrine, the practical conclusion at which surgeons seem to have arrived is, that a cancerous tumour is more dangerous as a centre of dissemination and as threatening suffering and death than useful as an organ of elimination, or as protecting other parts by concentration of the morbid tendency in itself, and that consequently its removal is, as a rule, desirable. A more safe and easy method of effecting this than the usual operation, without increased liability to recurrence, it is hoped is found in the injection of acetic acid. This alone would be no small gain, even if the disease is to be considered as constitutional. Cases would come under observation earlier, there would be no hesitation in interfering, as now very frequently happens, and the disease would not be allowed to gain head. But it has already, also, been found ot great use in cases which either from their situation or character were considered absolutely irremediable. The treatment is as yet in its infancy, and new developments of it will no doubt be made. It is probable that just as some of the salts of nitric, hydrochloric, or sulphuric acid are found useful as caustics, the destructive properties of the acid being limited, and the action modified by the base, so the acetates may find a place in the treatment of cancer—the peracetate of iron, for example. So, again, the chemical homologues of acetic acid may be found to have advantages over acetic acid itself. But at present the work lying immediately in our path is to ascertain what acetic acid will and will not do, and how to turn its effects on malignant structures to the best account. Failures are, of course, to be looked for; it is not to be expected that cancer, from being the one incurable disease, is to become by any treatment the one always curable.

As to the extension of this method of treatment, or modifications of it, to simple tumours, which naturally suggests itself, I do not think it promises well theoretically, and one has not the same strong inducement to try it.

There is one important class of cases, however, to which I should greatly wish to see some similar treatment applied—ovarian cysts. It is scarcely probable that I should, within any reasonable time, have the opportunity of making the experiment under the circumstances with a due regard to the safety of the patient would demand, or I should not content myself with suggestion. It may be thought that a dominant idea has overpowered my common sense, but it seems to me that the same considerations apply as in the case of cancer. The filling of an ovarian cyst is not the result of mere osmosis, but must be due to the activity of cells on its inner surface, while the extension and strengthening of the walls follows, on the principle, crudely expressed in the old physiological maxim, "*ubi stimulus ibi fluxus.*" To modify the structure and nutrition of these cells would, if it could be thoroughly effected, put an end to the growth of the cyst. This has been attempted by the injection of iodine, which, though not successful, has shown us that even strongly irritant fluids may be injected with impunity. I should not, as has been done in the case of iodine, empty the sac, but withdraw only a small amount of the contents—little more than would be replaced by the fluid to be injected, which would be, in the first instance, a small quantity of dilute acetic acid. No apparent effects, good or bad, following, I should repeat the injection in a week or ten days, testing the fluid withdrawn. When the contents of the cyst had become decidedly acid, I think it probable that the structure and vitality of the cells lining it would be affected and their growth arrested—a result which I should expect to be followed by resorption of the fluid and shrinking of the walls.—*Medical Times and Gazette, Nov.* 10, 1866, *p.* 512.

104.—ON THE ACTION OF ACETIC ACID ON OVARIAN CYSTS.

By Dr. F. E. Junker, London.

[In the preceding paper Dr. Broadbent speaking of ovarian disease says, "It seems to me that the same conditions apply as in the case of cancer." Dr. Junker denies this, and observes that the osmotic properties of the cyst must never be lost sight of. The chemical action of the acid will not remain confined to the secreting cells; such will also affect the stroma and the peritoneal coat of the cyst, and the surrounding parts. To determine these points Dr. Junker has made the following experiments.]

Acting on the suggestion of Mr. Spencer Wells, who kindly furnished me with the most appropriate material for the experiment—viz., a simple ovarian cyst, with a smooth external coat, perfectly free from adhesions, fresh, having been extirpated only last Saturday (10th inst.)—I constructed a dialyser by stretching a portion of the cyst as a diaphragm over a gutta-percha hoop; and after having carefully washed the cyst, I placed it in a vessel containing distilled water. Into the dialyser the same quantity of a mixture of two parts of water and one part of concentrated acetic acid (sp. gr. 1040) was placed. Both liquids were tested with litmus paper; at first at intervals of five minutes, and afterwards of fifteen minutes. Even the first test detected acidity in the distilled water in the vessel, and an increase of acidity steadily occurred. By comparing the two series of test-papers thus obtained after six hours, a marked decrease of acidity in the liquid within the cyst was observable, and after twenty-four hours the osmosis was completed, the test-paper showing the same degree of acidity within and without the dialyser.

But it was not merely osmosis that had taken place; there was also a perfect alteration and disintegration of the portion of the cyst-wall acted upon by the acetic acid. It had been swelled to considerably more than twice its original thickness. From being formerly an opaque fibrous stroma, it had now changed into a diaphanous gelatine, and softened to such a degree, that the mere weight of a goose-quill easily perforated it.

I am fully aware of the objection which might be raised, that an experiment with a dead membrane is vastly different from what would take place with a living membrane within the body. I can only say that osmosis, which plays such an important part in the vegetable and animal economy, will be carried on more rapidly through membranes in which circulation and active innervation perform their functions. The physiology of nutrition bears this out.

Experiments on animals must teach us what would be the local effect of acetic acid on the peritoneum and the serous coats of the abdominal viscera, even if its dissolving and destroying properties already alluded to did not exist. We must not forget that the peculiar anatomical relations of the cyst with the peritoneum in the abdominal cavity create very different conditions from those of a cancer surrounded by fibrous tissues, fasciæ, aponeuroses, muscles, and fat.

The results of my experiments have been quite enough to prove that injecting acetic acid into ovarian cysts must necessarily be a very dangerous practice. Not only osmosis of acetic acid into the peritoneum takes place—and we want experiments to show how the peritoneum would bear this—but the cyst-wall would become so rotten that rupture would be inevitable.—*Medical Times and Gazette, Nov.* 17, 1866, *p.* 543.

105.—ON THE INJECTION OF ACETIC ACID INTO OVARIAN CYSTS.

By Dr. W. H. Broadbent.

The results of the experiment made by Dr. Junker, for the purpose of showing the probable consequences of injecting acetic acid into an ovarian cyst, were exactly such as I should have anticipated, the conditions being given. I intend repeating it with the substitution of a portion of recent stomach for the cyst-membrane, and hoped to have done this already, but I have not had the opportunity. I will give the details next week, but I venture in the meantime to predict that the result will be similar; and it would be equally reasonable to conclude that taking vinegar as an article of diet is attended with danger to the integrity of the coats of the stomach, and involves the risk of dialysis into the peritoneal cavity, as that like consequences would attend the employment of dilute acetic acid by way of injection into ovarian cysts.

In the first place, the strength of the acid used in the experiment is altogether disproportionate to that of the acid which would act on the walls of the cyst if employed in the way I suggested. I did not, it is true, specify the degree of dilution, or the amount to be injected, but I think no one will fail to see that I should begin with small quantities of weak acid, which again would be diffused in the entire contents of the cyst.

Then, also, for dialysis, fluid is required on both sides of the membrane—a condition which would be met only by the presence of peritoneal fluid—an exceptional occurrence. But supposing the acid to diffuse into the peritoneal cavity, experiments of Dr. Richardson on the synthesis of disease with lactic acid furnish grounds for belief that no ill effects would follow. But Dr. Junker forgets entirely the important fact that during life blood is circulating in the walls of the cyst, and that dialysis takes place into it. This would prevent alike the diffusion of the acid into the peritoneal cavity and the gelatinisation of the cyst, just as it prevents the gastric juice from digesting the sto- the sac would thus not arise. On the other hand, the acid injected mach. The risk of peritonitis from contact of acid or rupture of into the cyst would be gradually taken up by the blood-vessels, slowly because of its admixture with so much organic matter, for which it has affinity. Dialysing through the inner layer of the cyst towards the blood, it would have on them, and only on them, an effect analogous to that described by Dr. Junker; and as these secrete (so to speak) the fluid contents, and retain more or less of the cellular character, it seems probable that the change of structure produced would modify their nutrition, and arrest their activity.

If a favourable opportunity offers, I shall certainly put this hypothesis to the test of experiment; and though I should take all precautions for the safety of the patient, I should entertain no fear of the consequences predicted by Dr. Junker.—*Medical Times and Gazette, Nov.* 24, 1866, *p.* 568.

106.—THE TREATMENT OF CANCER BY INJECTIONS OF NITRIC ACID.

By Dr. J. Hughes Bennett.

[In his work on cancerous and cancroid growths published in 1849, Dr. Hughes Bennett gives us the following passage, which shows that he was one of the first to point attention to the fact of cancer being acted upon by acetic acid and other agents.]

We have seen that certain chemical agents have a marked effect upon the cancer-cell. Acetic acid especially dissolves the cell-wall more or less, and strong potash reduces the whole to a granular mass. The continued application of these agents, therefore, would tend to dissolve the growth, if it could be brought into direct contact with the cells, and need not necessarily excite such irritation as to cause fresh exudation. The only objection is, the utter impossibility of affecting the whole mass even in cases of ulceration, and preventing the formation of deep-seated cells, while the superficial ones are destroyed. In certain cancroid growths, especially epithelial ones, the application of acetic acid is an established remedy, and should always be tried whenever it is thought possible to bring the fluid successively in contact with the entire mass of the disease. —*British Medical Journal, Nov.* 24, 1866, *p.* 593.

107.—ON THE TREATMENT OF CANCER BY THE CONTINUOUS GALVANIC CURRENT.

By Dr. Julius Althaus.

The treatment of cancer by subcutaneous disintegration, as proposed by Dr. Broadbent and others, having excited considerable interest in the profession, it may be useful to direct attention to another means of a similar character to those hitherto employed, and which, although it is not quite so easily to be had as the injection of acetic acid, yet will, no doubt, in course of time prove of the utmost value in the treatment of cancer and other tumours. I allude to the hitherto much neglected *chemical action of the continuous galvanic current*, apart from the calorific action of the same agent, which has long been known and used as the galvanic cautery.

Although Crusell, Ciniselli, and Tripier have at different times endeavoured to induce surgeons to make use of the chemical action of the continuous current in unmanageable cases of tumours, stricture, piles, &c., yet until now their efforts have not been crowned with success. This is, in a great measure, due to the circumstance that the batteries which have been employed were badly constructed for the purpose, and that the action of the current, when brought in contact with living tissue, was not thoroughly understood.

No such drawback now exists as regards the battery, since the modification of Daniell's arrangement which I described in the Lancet for August 12th, 1865, is the most perfect and reliable instrument for such cases that could be devised.

The knowledge of the chemical effects of the continuous current on living tissues has also now sufficiently advanced to enable us to apply this agent with perfect safety. Those who have hitherto operated with it have mostly committed the mistake of applying either both poles or the positive pole to the part to be removed, while *only the negative pole should be used in such operations*, the circuit being closed by placing the positive pole at a distance from the diseased part. M. Tripier is the only one who has as yet been alive to the advantage to be gained by employing the negative pole alone; but even he has recommended the use of the positive pole for vascular tumours and aneurisms!

At the *positive* pole acids are evolved, whereby the albumen is coagulated, and a clot is formed which firmly adheres to the needle used; while at the *negative* pole free alkali and hydrogen are developed, by which a slow and gradual disintegration of living tissue is brought about.

On removing the *positive* pole, hemorrhage may ensue; while the *negative* pole may be safely and easily removed without any untoward result.

The *positive* pole often produces inflammation after the operation, and the cicatrices left by it are hard and have a tendency to shrink considerably, thus leading to great disfigurement; while the parts acted upon by the *negative* pole form an eschar, which slowly separates, without pain or reaction, leaving a soft and yielding cicatrix.

The coagulating effect of the *positive* pole should therefore be entirely eschewed in all surgical proceedings; while the disintegrating effect of the *negative* pole may be safely employed wherever a needle or a sound can be introduced.

I have already had an opportunity of testing the accuracy of these principles in a case of non-cancerous tumour, which occurred some time ago in the practice of an eminent surgeon, and in which this method of operation proved entirely successful. The details of this case will shortly be placed before the profession.

I feel convinced that cancerous as well as other tumours can be removed by the continuous current, almost without pain, and without subsequent constitutional disturbance.—*Lancet, Nov.* 24, 1866, *p.* 597.

108.—HOW FAR IS THERE GROUND FOR SUPPOSING THAT THE WATER-SUPPLY IS A CAUSE OF THE SPREAD OF CHOLERA IN LONDON?

[The following article is from the Report of the Lancet Sanitary Commission on the Epidemic of Cholera in the East end of London. The evidence which we have already that the spread of cholera among a population may be influenced by the nature of the drinking water is first set forth in a general way.]

"An experiment, at which mankind would have shuddered if its full meaning could have been prefigured to them, has been conducted during two epidemics of cholera on 500,000 human beings. One-half of this multitude was doomed in both epidemics (1848-49 and 1853-54) to drink the same fecalized water, and on both occasions to illustrate its fatal results; while another section, freed in the second epidemic from that influence which had so aggravated the first, was happily enabled to evince by a double contrast the comparative immunity which a cleanlier beverage could give."*

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* Mr. Simon's Report on the last two Cholera Epidemics of London as affected by the Consumption of Impure Water. Addressed to the Right Hon. the President of the Board of Health.

The unfortunate people who were experimented upon were the inhabitants of nine districts of London, south of the Thames —namely, of St. Saviour's, St. Olave's, and St. George's, Southwark; of Bermondsey, Newington, Lambeth, Wandsworth, Camberwell, and Rotherhithe. One part of this people were supplied by the Southwark and Vauxhall Company with water from the Thames at Battersea during both epidemics; the other part of this people, intimately distributed through the first part, were supplied by the Lambeth Company with water from the Thames at Battersea during the epidemic of 1848-49, and with water from the Thames at Thames Ditton during the epidemic of 1853–54. The following were the results of this experiment:—

First. "In the 24,854 houses supplied by the Lambeth Company, comprising a population of about 166,906 persons, there occurred 611 cholera deaths, being at the rate of 37 to every 10,000 living. In the 39,726 houses supplied by the Southwark and Vauxhall Company, comprising a population of about 268,171 persons, there occurred 3,476 deaths, being at the rate of 130 to every 10,000 living. *The population drinking dirty water accordingly appears to have suffered three and a half times as much mortality as the population drinking other water.*

Second. The tenantry of the Lambeth Company (a population of 166,906, comprised in 24,854 houses, as has been just mentioned) lost 611 persons by the epidemic of 1853-54; while by the epidemic of 1848-49, in the same houses (or rather in as many of them as then existed), it lost 1,925 persons. Not only then, did this population suffer not a third as much as its neighbours in 1853 54, but it suffered also not a third as much as at the time of its unreformed water-supply (1848-49).

To this evidence of a positive kind, as most of our medical readers are aware, much could be added, were it necessary. But we shall now proceed to a consideration of the water-supply of the East of London.

The east of London north of the Thames is supplied with water exclusively by one company—the East London—to a line running irregularly northwards from the Thames at St. Catherine's Docks. This line crossing the basin, and passing between the West and East Dock, makes a detour round the Mint at some distance east of it, and then proceeds westward a short distance, south of the Blackwall Railway. It turns northward again along Mansell-street, Somerset-street, across High-street, Whitechapel, and along Goulston-street. At this point it passes with slight irregularity to the south end of Norton Folgate, it then proceeds along Norton Folgate, Shoreditch, and the beginning of the Kingland-road. Just beyond where the North London Railway crosses the Kingsland-road it leaves this road and proceeds in a straight line in a north north-west direction for about a mile and three-quarters, to where the Amherst-road and Dalston-road cross. At this point it bends in a north-westerly direction to follow some distance east of them—the Amherst and Rectory-roads; but we have followed it far enough for our purpose. We should have mentioned that in the course of this line along Shoreditch it leaves the main road for a time so as to include a semicircular mass of houses facing the terminus of the Great Eastern Railway.

The southern portion of this line corresponds to a certain extent with the boundary between the Central and Eastern Registration Districts. The central districts are supplied by the New River Company. In the week ending July 21st more than six-sevenths of the deaths by cholera occurred in the eastern districts, and in that ending July 28th more than eight-ninths. In the former week, 309 out of 346 deaths by cholera happened in the registration districts of London; in the latter week 815 out of 904 in the field of the East London Water Company's mains.

If we take the boundary line we have indicated of the East London Water Company's mains we find that in Aldgate, which lies across the south of this line, of the six deaths which occurred up to July 28th, five were on the East London Company's side of the line. North of the Aldgate sub-district is that of St. Botolph on the west side of the line, and of Goodman's-fields on the east side. In the former sub-district, which is supplied by the New River Company, and perhaps Bishopgate and Whitechapel pumps—the last of which sources is in a terrible condition—four deaths were registered. This we shall see is the only marked instance of the spread of cholera immediately west of the boundary line. In Goodman's-fields, supplied by the East London Water Company, seventeen deaths by cholera have occurred up to July 28th. In Whitechapel Church sub-district, north of this, and, like it, east of St. Botolph's, seventy-nine cholera deaths have been recorded. North of this district lies the Whitechapel North sub-district, in which twelve deaths have occurred up to July 28th. This district extends a little west of the boundary line of the East London Company's mains, but all the deaths occurred east of the line. In Whitechapel, Artillery sub-district, three cholera deaths, up to July 28th, have been recorded on the east side of the line. North of the St. Botolph sub-district and west of the East London Water Company's mains occurs the Holywell sub-district, and north of this the St. Leonard's sub-district, both in the East registration district, but west of the mains of the East London Water Company. In the Holywell sub-district two deaths by cholera, and in the St. Leonard's sub-district one, occurred up to July 28th.

North of the Whitechapel Artillery sub-district comes the Bethnal-green, Hackney-road sub-district, east of the last mentioned sub-districts, and supplied by the East London Water Company. Here eleven deaths have occurred up to July 28th. North of the preceding, and running across the limits of the East London Water Company's mains, is the Haggerston West sub-district; in this one death has been recorded, occurring east of the limiting line.

These points in the distribution of the cholera deaths occurring near and on either side of the limits of the East London Water Company's supply we have made out by comparing the Registrar-General's returns with a map, on which the engineer to the company has kindly traced for us the limiting line. From a consideration of these points, it will be seen that this line indicates with very general accuracy the boundary of the cholera field in London. Further, the cholera field extends beyond the London registration districts, to West Ham, Plaistow, and Stratford; and so does this company's water-supply. However, on the other hand, their mains run fairly north of the present cholera field. The evidence so far seems to bear strongly against the East London water. Still, it may be only an accidental coincidence that the cholera and the water supplied by the company should be distributed over the same area. With regard, however, to the lowness of the district where the disease prevails, other regions in South and Central London are as low. And while, as to drainage, the incompleteness of the low-level sewer leaves a large part of the cholera field undrained, this is not so, we believe, in the Bethnal-green district, which is drained by the mid-level sewer, and in which 137 deaths from cholera have occurred up to July 28th.

We have gone over the waterworks at Old Ford, under the guidance of Mr. Greaves, the engineer, who kindly placed himself at our disposal for the time, and readily gave us such information as we sought. There is at Old Ford a large covered reservoir, which is in communication with the company's filter-beds at Lea-bridge by means of a four-foot iron pipe, which runs under ground on the east side of the Hackney-cut. From this reservoir is sent the greater part of the supply of water to the low-lying districts of the company's mains (the present cholera field.) There are also at Old Ford two large open reservoirs, communicating with the Lea-bridge works by an open canal. These reservoirs and this canal lie east of the Hackney-cut. This canal and the open reservoirs are not used, we were told, at present for the water-supply although they communicate with the covered reservoir, and therefore can be used on an emergency. This fact is much to be regretted, although it may be the case that this emergency never does occur. The covered reservoir lies west of the Lea, in what we cannot but feel to be dangerous proximity to the river, under which the iron feed-pipe from the filter-bed has to pass. However well the reservoir may be made, it is very difficult to keep water from oozing through brick and concrete. The water in the reservoir looked very clear, and exhaled nothing but a pure-smelling aqueous vapour. The latter statement would, however, we hear, not always be true. The open reservoirs have not long since been cleansed ; and we are informed by an eye-witness from the Great Eastern Railway, which passes along their margin, that the matters removed were very offensive. From the filter beds at Lea-bridge the water is supplied direct to the mains of the high-lying district, the excess supplied flowing down to assist in supplying the low-lying districts. This may serve to explain the non-extension of the cholera field to the north and high part of the East London water field. Of course it can only do so on the condition that the water received in the lower districts differs in quality from that in the upper, either by the supply to the upper districts being purer, or by deposits having collected in the lower mains, and being swept on with the water when it is turned on to the houses. At any rate the connexion of the difference of supply with the difference in the prevalence of cholera is an important fact to be noted.

The East London Water Company take all their supply, we undersand, for their filter beds at Lea-bridge from the river Lea at Ponder's-end, more than two miles above the bridge, by a cutting running down to their works ; but their works have also a communication with the river at Lea-bridge by means of wired lock-gates which rudely strain the water which flows through them.

The sewage of Edmonton, the refuse from a gasworks and from a crape manufactory, are received into a cutting made by the company to intercept its course into the river until it gets to below Tottenham where, together with the sewage of this place, it then enters the river—that is, at the point above Lea-bridge, but below Ponder's-end. At Ponder's-end, our informant tells us, the water seems clear and good ; near and above Lea-bridge carcases of animals were observed in the water. It is needless for us to mention the filthy state of the river below Lea-bridge.

Now with regard to the state of the water. We must for present information on this matter turn to the analysis made under the superintendence of Professor Frankland, and published by the Registrar-General. We shall convert the numbers representing parts in 100,000 of water into the more familiar statement of grains per gallon.

East London Water.	Total solids per gallon.	Organic and other volatile matters.	Oxygen required to oxidize the organic matter.
Average of a year ...	19·55 grs.	1·13 grs.	·0353 grs.
Collected July 1st ...	17·07 ,,	1·36 ,,	·0241 ,,
Collected Aug. 1st ...	18·30 ,,	1·01 ,,	·0230 ,,

These results can hardly be said to afford any very important indication. For while the organic and other volatile matters were so largely above the average quantity on July 1st, the oxidizability of the water was at that time below the average. On August 1st both the amount of the organic matters and the oxidizability of the water were below the average, particularly the latter. The condition of the water as indicated by these results is as good as that of the Thames Companies' supplies, but it is decidedly inferior to that of the New River Company, which derives its supply in part from the Lea.

The presence of organic matter in water is no actual proof of its unfitness for human consumption; much less can the relative quantity of organic matter be taken as an indication of the degree of its unfitness. It is the *nature* of the organic matter which is of importance. These matters, or the product of their change, may interfere with the healthy action of the body without exciting any specific disease, or they may be the means of conveying a contagious disease. In the latter case they must of course be of animal origin, and usually of human origin. It is in this way that water is believed to excite cholera—namely, by conveying organic particles from a cholera patient to another person. From this possibility of the organic matter derived from human beings being capable of exciting in a person the disease of him they come from, they are to be looked upon with exceeding suspicion and dread. Besides which, they usually possess the putrefactive quality to a very much greater extent than organic matter derived from the vegetable kingdom, so as to be largely armed with all the deleterious power of this decaying vegetable matter.

The surest way of determining the human or other origin of the organic matter found in a sample of water is to trace the history of the water. But at times microscopic observation serves to detect animal matter in water, if it be in the solid state; while the detection of nitrogen in the organic matter, or of this substance, as ammonia or nitric acid in material quantity in the water, renders the past or present occurrence of animal matter in the water probable. As a rule, the detection of nitrogenous organic matter in water may be considered to prove it to be a noxious beverage. The presence of ammonia or nitric acid does not indicate that the water has baneful properties, but it shows that nitrogenous (probably animal) matters find their way into the source from which the sample was taken, and the danger therefore of using such water for fear that at any time the change of these matters into such harmless bodies as ammonia and nitric acid may not have taken place.

The atmosphere is the great purifier of water, partly by the chemical action of its oxygen and ozone, and partly by its removing gases and vapours from the water by diffusion. But there is no certainty as to what degree of exposure is sufficient thus to purify a water. Many persons consider the atmospheric purification of water to take place very rapidly, while Sir Benjamin Brodie, Professor of Chemistry at Oxford, in his evidence before the Royal Commission appointed to inquire into the Pollution of Rivers, states his opinion to be that it is simply impossible for the oxidising power of the atmosphere over sewage running in mixture with water over a distance of any length to be sufficient to remove its noxious quality, and that the noxious quality imparted to the Thames water by the sewage of Oxford is not removed by the time this water reaches Teddington Lock (above which the Thames companies draw their supplies for London). This, it must be remembered is the opinion of one of our most profound chemists, but we cannot but feel that in it the powers of the atmosphere are decidedly underrated.

There is one property which all physiology serves to show organic matter must possess to be active in developing vital actions, and that is a tendency to undergo *change*—the property of chemical instability. Now this property is possessed by organic matter—more especially animal matter—pretty nearly in proportion to its power of undergoing oxidation. Hence, matter not easily oxidized can have but little action on the body, so that its presence in water is of comparatively little importance, while the presence of readily oxidizable water is of correspondingly great importance. Now readily oxidizable organic matter will often take oxygen from things that it meets containing it—converting, for instance, sulphates into sulphides, and nitrates into nitrites. So that the presence in water of these salts and other allied bodies, as well as of oxidizable matters, is a probable evidence of the powers of the water to cause some effect on the human organism for good or for evil, and much more likely for the latter. The oxidizability of the constituents of a drinking-water is therefore a material point to determine, and forms an important part of the examination of a drinking-water. It is since the outbreak of cholera in 1853–54 that an available means of doing this has been made out. In respect of this point, the East London water contrasts favourably with that of the Thames Companies.

Before closing our present remarks on this part of the subject we are anxious not to pass over the methods we have of freeing our drinking-water from possibly dangerous impurities. Thorough filtration is indispensable, because not only does it remove some of the impurity, but probably some of the most active part of it, since different considerations serve to render it likely that solid particles of organic matter are more powerful for harm than organic matter in solution. But filtration alone is insufficient. Boiling the water is a well-known means of purifying it; but this, besides usually rendering the water vapid to the palate, cannot, in the opinion of many scientific men, be relied on.

The Registrar-General's report contains two recommendations from Professor Frankland—one, the admixture of permanganate of potash with the water; the other, the use of animal charcoal filters or purifiers at the waterworks. He calculates that 300 tons of bone black would be required between all the metropolitan water companies, which would serve for at least three months—perhaps for a year—to remove the whole of the organic matter. Its employment he considers to be neither formidable nor expensive. He calculates that if the permanganate be employed, 80 ℔. ought to be mixed with 1,000,000 gallons of water—a calculation that indicates the requisition of 8000 ℔s. of this substance per diem to purify the water supplied to London. This method would not only be of enormous expense, but it would render the water turbid should any of the permanganate pass unchanged through the filter-beds.

Our own recommendation is to employ Mr. Spencer's "magnetic carbide"—a form of magnetic oxide of iron prepared by him. This substance is stated by him to cause the oxidation of all the organic matter in water without loosing its power by use. It is equally applicable, we believe, to the filter-beds of a waterworks and to the house-filterer. Of our own knowledge of its power we can speak thus far: one of his domestic purifiers *thoroughly* oxidized the water supplied by one of the Thames companies, and this after a month or two's use as well as at first. The test of oxidizability of the water before and after purification was the permanganate of potash solution. The colour of the water caused by the organic matter was also removed by this purifier. In this material, then, lies a means of purifying water at a trifling cost at the waterworks. And in Spencer's water purifiers for domestic use (obtainable at Euston-Square), or in the filterers of the London Water-purifying Company (Strand), as mentioned by Professor Frankland, such persons who can afford the necessary outlay have what may be looked upon as a safeguard against drinking-water containing oxidizable organic matter.—*Lancet, Aug.* 11, 1866, *p.* 157.

109.—CHOLERA AND IMPURE WATER.

By Dr. F. P. Atkinson, Rochester.

It has been clearly shown, I think, that the present outbreak of cholera has been due in a great measure to the supply of bad water, and there is good reason to believe that the same cause has been at work in nearly all, if not all, previous epidemics. There was one remarkable instance, at any rate, in the epidemic of 1849. The Wandsworth-road, as it is well known, was most severely visited at that time, and one part of the eastern side was attacked with such virulence that in a short time there was not a house where there was not one dead. In some cases there was not one person living, and the dead were actually left to bury their dead. The opposite side of the road, however, scarcely suffered at all, which led people to guess that the origin of the disease was to a certain extent local: and when they found that the two sides were supplied by two different water companies, they naturally enough began to blame the water. As it turned out afterwards, they were perfectly correct; but they little imagined they had all along been poisoning the water themselves. On examination of the cisterns it was found that the pipes which ought to have conveyed water solely for the purpose of cleansing the closets, allowed the excreta, &c., on certain occasions to regurgitate and infect the water which was used for drinking and other domestic purposes. This was the origin of the cholera on the eastern side, and the same cause not being at work on the western side, it escaped. Of course the bad water, *per se*, was not the originator of the disease, but it acted as a predisposing cause, and rendered the people more liable to it when the time came.—*Lancet, Sept.* 29, 1866, *p.* 371.

110.—CASE OF SPASMODIC CHOLERAIC DISEASE SUCCESSFULLY TREATED BY HOT-WATER PACKING.

By F. A. Bulley, Esq., Senior Surgeon to the Royal Berkshire Hospital, Reading.

Thomas P., aged 40, a county police-constable, residing at Maiden Erleigh, near Reading, a temperate man, and previously in good health, was attacked while walking on his beat, about one o'clock p.m., on Thursday, August 2nd, with sudden violent pain in the abdomen, followed almost immediately by tonic painful spasm of the abdominal muscles, and a sensation as if his intestines were being drawn up in knots. It was not accompanied by any particular feeling of sickness, nor had he had any previous diarrhœa. The symptoms had been gradually increasing in severity until I saw him — about half-past three the same day—when I found him writhing with the most excruciating pain. The abdominal muscles were universally hardened and contracted by spasm, somewhat resembling tetanic spasm, but more continuous and unrelaxing, and extremely sensitive to the touch. The most intense pain was just in the epigastric region, extending backwards to the spine, as if the diaphragm was affected with the spasm. He was in a cold, clammy perspiration, pulse feeble, and he was beginning to feel extremely faint. I immediately ordered the abdomen to be fomented, and the flannels, wrung out of water as hot as he could bear, having been applied three or four times, his body was enveloped in blankets to the number of five or six, the outer one being closely wrapped round his neck, to confine the heat generated by the wrapping, and to take the following draught every two or three hours:—℞. Elixir opii (Newbery's) ♏x.; tinct. capsici ♏v.; olei menthæ virid., olei anisi, āā ♏ij.; confect. aromat. ʒj.; aquæ menthæ pip. ad ℥jss. M. ft. haust. To take a little brandy-and-water occasionally.

On visiting him about two hours afterwards, I found him lying in a profuse perspiration, which, with the medicine, had much relieved, but not quite removed, the pain. The rigidity of the abdominal muscles had greatly subsided, and he altogether felt much more comfortable.

About 7 p.m. he had another attack of pain, but not by any means so severe as the first, for which his wife again fomented him and repeated the medicine, which again relieved him, some little amount of abdominal hardness, however, continuing. About 12 at midnight he had another attack, when his wife again packed him up in the wrappings and repeated the mixture. Shortly after this, while in the perspiration induced, he fell asleep, and on awaking about an hour afterwards he found the pain had left him, the abdominal spasmodic hardness had become quite relaxed, and he felt in every respect much better.

Friday, next morning. He is quite free from pain and spasm; feels very weak, but the pulse has recovered its strength, with a natural uniform warmth of the body. In the afternoon he was apparently free from all symptoms of the disease, except that he felt somewhat languid and faint. Ordered to take cold strong beef-tea and small quantities of brandy-and-water for drink, and the following mixture should the pain or spasm return:—℞. Chlorodyn. ♏xl.; aquæ menthæ virid. ad ℥vi. M. ft. mist. Take a fourth part if necessary.

Next day the patient had apparently quite recovered from his complaint.—*Medical Times and Gazette*, *Aug.* 11, 1866, *p.* 139.

111.—CASE OF ASIATIC CHOLERA SUCCESSFULLY TREATED BY NITROUS ACID; WITH REMARKS.

By Dr. G. E. Nicholas, Medical Officer of Health for Wandsworth.

[In 1850 cholera was continuously present for six months in the Mediterranean squadron, and every mode of treatment, eliminative or astringent, was tried and found of no use except that by nitrous acid.]

During the epidemic in this country in 1854 it underwent at my hands, and at those of my colleague, Dr. Whiteman, of Putney, the rigid test of a general administration. The results of treatment in my subdistrict were as follows--viz., in 659 cases of diarrhœa, choleraic diarrhœa, and cholera, there were fifty-eight cases of collapse of which five were moribund when visited, and consequently not under treatment, and twenty-nine recovered.

This is not a time for founding a theory of rational Medicine upon which to build an experience. Experience of treatment and of remedies must seek—though, as is too often the case in Medicine, it cannot command—an explanation of their *modus medendi.*

Whether the eliminative mode of treatment be right in theory but wrong in practice, or whether, conversely, the suppression of the diarrhœa be wrong in theory but salutary in practice, may be left to the mental profundity of censors of Colleges; but, as a matter of fact and experience, there must be many by this time whose experience of its use will bear out the assertion that the nitrous acid will speedily arrest the diarrhœa and its concomitant symptoms, and that such suppression is never followed (in uncomplicated cases) by the very unmanageable consecutive fever which usually occurs when it has been effected by opium alone or combined with the usual astringents; and, moreover, that in cases of collapse, when vomiting and purgation are at an end, it will cause reaction when no other remedy—not even castor oil—will avail to do so.

The formulæ I have used are the following, viz. :—℞. Acidi nitrosi ʒj. (the deeper the colour the better); aquæ ℥viij. M. ft. mist., cujus cochl. magnum ex aquæ cyatho vinosa post singulas sedes liquidas sumendum est. Si sit dolor, adde tinct. opii ʒj.; si sint spasmi, ætheris chlorici ʒj. Sed si sit vomitus, acidi gallici gr. ij. ad iij. imprimis sumenda sunt.

As an illustration of this treatment, I beg to add the following short notes of the only case of cholera which has come under my notice in this subdistrict up to the present time.

Case.—T. R., aged 61, labourer, employed lately in digging mud from the bed of the Wandle, &c., and previously in the manufacture of artificial manure. On Sunday, August 12, was taken, at 4 a.m., with vomiting and purging incessantly. Had been drinking overnight. ℞. Acidi gallici gr. ij., c. pulv. Doveri gr. v., st.; mist. acidi nitrosi, ut opus sit; turpentine fomentation to stomach, and hot water and turps to the feet. 1.30. Cold surface; pulse scarcely perceptible, cannot be counted; speaks in a whisper; skin dusky blue; features shrunken; stools rice-water, but pinkish from admixture of blood, like sanious lymph; cramps very severe; tongue has a thick white fur. Repetantur remedia omnia. 3 p.m. Has had four motions, and been sick three times. Pulse 120; more heat of surface; less cramp. 7 p.m. Better; pulse 100, fuller; has vomited three times, and been purged four times, but in less quantity; warmth of the skin restored; skin bluish red in colour; voice louder. To have some thick arrowroot and brandy, and raw extract of meat, and plenty of it, but little at a time. Continue remedies. 10 p.m. Informed by message that he was still improving, and had been purged but little. Medicines to be used as required.

August 13, 11.30 a.m. Has passed a restless night; four stools, the last has fæcal odour, and blackish from the gallic acid (no doubt); sick once this morning; passed a little water at 10 a.m.; pulse 90, fuller; skin dark red; tongue cleaning. 5 p.m. Going on favourably. Continue mixture as needed; same nourishment.

14th. Pulse 85; still weak; has had five stools, but small in quantity, evidently contain bile; feels comfortable; no cramp since yesterday, and no sickness. Continue.

15th. Progressing favourably. Pulse 72; tongue cleaner. Continue.

16th. Progressing. Continue; four ounces port immediately.

17th. Convalescent, but very weak. Continue same diet &c.

18th. Suffering from nothing but debility. Fish, yolk of egg, milk, and gravy-tea for diet. Continue wine.

20th. Stronger. To have mutton in place of gravy-tea.—*Medical Times and Gazette, Aug.* 25, 1866, *p.* 193.

112.—CASE OF CHOLERA WITH COLLAPSE—TREATMENT BY CASTOR OIL—RECOVERY.

By Mr. SHAW, House-Surgeon to the St. Pancras and Northern Dispensary.

[The patient was a woman forty-five years of age. When first seen, on July 20, she was in a state of collapse, with cramps in the feet and legs, and to a less degree in the stomach and bowels.]

Ordered to have hot applications to the surface of the body and extremities, and the following mixture :—℞. Olei ricini ʒvj., mist. acaciæ ad ℥iij.; half to be taken every two hours. Warm tea was given to drink, but all alcoholic stimulants were prohibited. 10 p.m.—Been sick after each dose of the medicine, the vomit being brown and very fluid; bowels opened three times, the evacuations very fœtid, and not a trace of bile in them; no urine passed; difficulty of respiration very great. Ordered — Olei ricini fl.ʒiv., mist. acaciæ fl.℥ij.; half to be taken every two hours. Owing to the great thirst, iced water was given to drink. 12 p.m.—Had retained the first dose of the last mixture, but vomited the second; bowels opened once since 10 p.m., no bile in the evacuation; great depression; pulse 84. Ordered to have beef-tea and the following mixture: ℞. Olei ricini fl.ʒiij., mist. acaciæ fl.℥iij.; one third to be taken every two hours.

July 21, 6 a.m. Skin much warmer; blueness of face gone; no cramps, and respiration much easier; bowels opened twice since midnight; there was some yellow bile in the last motion; vomited green bile once since last night; pulse 90; no cold sweating; passed some urine; ordered to have more beef-tea, and to continue the iced water. 4 p.m.—Going on very comfortably; bowels opened once since 6 a.m.; much biliary colouring in the evacuation; no vomiting; skin warmer; no cramps; complains of great "weakness;" voice much stronger. Ordered—Quiniæ sulph. gr. ij, acid. sulph. dil. ♏vj. aquæ fl. ℥j. To be taken every four hours.

The evacuations after this date were of a much more natural colour, and the patient, though much exhausted, continued to progress favourably; the voice finally recovered its natural force and quality.—*Medical Times and Gazette, July* 28, 1866, *p.* 89.

113.—RESULT OF THE TREATMENT OF NINETEEN CASES OF CHOLERA BY CAMPHOR.

[The following particulars are reported by Mr. Widdas, who, along with Mr. Richardson, had charge of the cholera patients at the Whitechapel Workhouse Infirmary.]

During the last twelve days nineteen of the cholera cases admitted into this union have been treated with camphor; of this number, six have recovered, nine have died, and the remainder are at present under treatment.

We administer the camphor by dissolving it in an equal weight of chloric ether, giving fifteen or twenty drops of the solution on sugar in the first instance, and ten drops every quarter or half-hour afterwards, according to the urgency of the symptoms. In about half the cases in which this treatment has been adopted the patients have appeared to receive benefit in periods varying from six to eighteen hours. The extremities have gradually become warmer and lost their lividity, bile has reappeared in their evacuations, and the patient has passed into a state of reaction. During this second stage two died—one, a girl aged 18 (who was suffering from severe gonorrhœa), with symptoms of congestion of the brain, and the other of pneumonia. Six have gradually recovered, without any bad symptoms.

Of the remaining half which died some were almost in a dying state when admitted, and one was suffering severely from secondary syphilis. And in some cases, even when the treatment was begun early, the camphor did not seem to have the slightest good effect, and the patients died collapsed in a very few hours.

Brandy has been freely administered to each, and ice has been given to suck, also effervescing mixtures and astringents incidentally.

All have been well-marked cases of Asiatic cholera, having the rice-water evacuations, coldness and lividity of the extremities, and in most cases the patients were pulseless when the treatment was commenced. In one woman, aged 57 years, after recovering from a state of collapse, and about four days from the commencement of the attack, a rash was observed all over the body, and most resembling that of measles, but on the following day the rash assumed a bright rosy red colour, strongly contrasting with the dusky red in measles. The rash remained about three days, and gradually disappeared. The woman is fast recovering. I may mention that all these cases have been adult women, varying in age from 18 to 70 years.—*Medical Times and Gazette, Aug.* 4, 1866, *p.* 118.

114.—SUCCESSFUL TREATMENT OF CHOLERA.—CAMPHOR.

Mr. **Frederick Smith**, of York House, Penzance, writes to us detailing a method of treatment, pursued at Naples, by which in 592 cases not a single death occurred.

Happily, the remedy in question is one which has been employed by all the medical schools, and can give rise to no angry debate. The dominant school, sometimes called allopathic, have used it; and that comparatively small section of the profession called homœopathic is also in the habit of prescribing it. But neither party, as a school, has thought of preparing it in the way in which Dr. Rubini—the author to whose labours in this cause I am about to refer—has found so invariably successful. For example, the ordinary spirits of camphor of the British Pharmacopœia consists of one part by weight of camphor to nine parts of spirits of wine; the homœopathic preparation of one of camphor to five of spirits of wine; whereas, Dr. Rubini's preparation consists of equal parts by weight of camphor and spirits, and to the power thus obtained he attributes his success in the treatment of the disease. And here I may say that the quantity of camphor which water will take up is small. To obtain, therefore, the "saturated spirits of camphor of Rubini," it is necessary to distil spirits of wine, and get rid of so much of its water as will bring it to 60° overproof, in which condition it will dissolve and hold in solution its own weight of camphor.

With this "saturated spirits of camphor" Dr. Rubini, an eminent Neapolitan physician, has treated in Naples 592 cases of Asiatic cholera without the loss of a single patient. Of these 592 cases 200 were cured in the Royal Alms House, 11 in the Royal Poor House, and 166 in the Third Swiss Regiment of Wolff. That the 377 cases treated by Dr. Rubini in these public institutions were all genuine cases of Asiatic cholera, and some "terribly severe," and that all recovered, the evidence of the following distinguished individuals, with their official seals attached, sufficiently attest:—Il Generale Governatore Ricci, Il Maggiorie Commandante Nicola Forni, Il Capitano Commandante Carlo Sodero, Generale Commandante Fileppo Rucci, Colonel Eduardo Wolff.

"The method of cure" is as follows:—"When a man is seized with cholera, he should at once," says Rubini, "lie down, be well wrapped up in blankets, and take every five minutes four drops of the saturated tincture of camphor. In very severe cases the dose ought to be increased to from five to twenty drops every five minutes. In the case of a man of advanced age, accustomed to take wine and spirits, where the drug given in drops has no effect, give a small coffee-spoonful every five minutes, and in a very short time the coveted reaction will occur. Ordinarily, in two, three, or four hours abundant perspiration will come out, and then cure will follow."

"The preventive method," writes Dr. Rubini, "is this, let those who are in good health, while living in accordance with their usual habits, take every day five drops of the saturated spirits of camphor upon a small lump of sugar (water must never be used as a medium, or the camphor will become solid, and its curative properties cease), and repeat the dose three or four times a-day. Spices, aromatic herbs, coffee, tea, and spirituous liquors should be avoided."—*Medical Press and Circular*, *Aug.* 2, 1866, *p.* 140.

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